# PHILOSOPHICAL

# TRANSACTIONS,

OF THE

# ROYAL SOCIETY

OF

# LONDON.

FOR THE YEAR MDCCCV.

PART I.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, BOOKSELLERS TO HIS MAJESTY,
AND PRINTERS TO THE ROYAL SOCIETY.
MDCCCV.

# PHILOSOPHICAL TRANSACTIONS,

OF THE

# ROYAL SOCIETY

OF

# LONDON.

FOR THE YEAR MDCCCV.

PART I.

LONDON.

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, BOOKSELLERS TO HIS MAJESTY.
AND PRINTERS TO THE ROYAL SOCIETY.
MDCCCV.

# ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of



27676

treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

# CONTENTS.

I. *The Croonian Lecture on muscular Motion. By* Anthony Carlisle, *Esq. F. R. S.* page 1

II. *Experiments for ascertaining how far Telescopes will enable us to determine very small Angles, and to distinguish the real from the spurious Diameters of celestial and terrestrial Objects: with an Application of the Result of these Experiments to a Series of Observations on the Nature and Magnitude of Mr.* Harding's *lately discovered Star. By* William Herschel, *LL. D. F. R. S.* p. 31

III. *An Essay on the Cohesion of Fluids. By* Thomas Young, *M. D. For. Sec. R. S.* p. 71

IV. *Concerning the State in which the true Sap of Trees is deposited during Winter. In a Letter from* Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.* p. 88

V. *On the Action of Platina and Mercury upon each other. By* Richard Chenevix, *Esq. F. R. S. M. R. I. A. &c.* p. 104

VI. *An Investigation of all the Changes of the variable Star in* Sobieski's *Shield, from five Year's Observations, exhibiting its proportional illuminated Parts, and its Irregularities of Rotation; with Conjectures respecting unenlightened heavenly Bodies. By* Edward Pigott, *Esq. In a Letter to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.* p. 181

VII. *An Account of some analytical Experiments on a mineral Production from Devonshire, consisting principally of Alumine and Water. By* Humphry Davy, *Esq. F. R. S. Professor of Chemistry in the Royal Institution.* page 155

VIII. *Experiments on Wootz. By Mr.* David Mushet. *Communicated by the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.* p. 163

## APPENDIX.

*Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.*

### ERRATA.

Page 4. line 8 from the bottom, *for* accipenser, *read* acipenser.
11. — 20, *for* their, *read* its.
26. — 7, *read* 6 ounces of water.
76. — last but one, *for* ,0058, *read* ,054.

# PHILOSOPHICAL

# TRANSACTIONS.

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the Year 1804, the Medal on Sir GODFREY COPLEY's Donation, to SMITHSON TENNANT, Esq. F. R. S. for his various Chemical Discoveries, communicated to the Society, and printed in several Volumes of the Transactions.

And they adjudged the Gold and Silver Medals, on the Donation of BENJAMIN Count of RUMFORD, to Mr. JOHN LESLIE of Largo, for his Experiments on Heat, published in his Work, entitled an Experimental Inquiry into the Nature and Propagation of Heat.

# PHILOSOPHICAL TRANSACTIONS.

## I. *The Croonian Lecture on muscular Motion.* By Anthony Carlisle, *Esq. F. R. S.*

Read November 8, 1804.

ANIMAL physiology has derived several illustrations and additions, from the institution of this Lecture on muscular Motion; and the details of anatomical knowledge have been considerably augmented by descriptions of muscular parts before unknown.

Still, however, many of the phenomena of muscles remain unexplained, nor is it to be expected that any sudden insulated discovery shall solve such a variety of complicated appearances.

Muscular motion is the first sensible operation of animal life: the various combinations of it sustain and carry on the multiplied functions of the largest animals: the temporary cessation of this motive faculty is the suspension of the living powers, its total quiescence is death.

By the continuance of patient, well directed researches, it is reasonable to expect much important evidence on this subject;

and, from the improved state of collateral branches of knowledge, together with the addition of new sources, and methods of investigation, it may not be unreasonable to hope for an ultimate solution of these phenomena, no less complete, and consistent, than that of any other desideratum in physical science.

The present attempt to forward such designs is limited to circumstances which are connected with muscular motion, considered as causes, or rather as a series of events, all of which contribute, more or less, as conveniences, or essential requisites, to the phenomena; the details of muscular applications being distinct from the objects of this lecture.

No satisfactory explanation has yet been given of the state or changes which obtain in muscles during their contractions or relaxations, neither are their corresponding connections with the vascular, respiratory, and nervous systems, sufficiently traced. These subjects are therefore open for the present enquiry, and although I may totally fail in this attempt to elucidate any one of the subjects proposed, nevertheless I shall not esteem my labour useless, or the time of the Royal Society altogether unprofitably consumed, if I succeed in pointing out the way to the future attainment of knowledge so deeply interesting to mankind.

The muscular parts of animals are most frequently composed of many substances, in addition to those which are purely muscular. In this gross state, they constitute a flexible, compressible solid, whose texture is generally fibrous, the fibres being compacted into fasciculi, or bundles of various thickness. These fibres are elastic during the contracted state of muscles after death, being capable of extension to more than one-fifth

of their length, and of returning again to their former state of contraction.

This elasticity, however, appears to belong to the enveloping reticular or cellular membrane, and it may be safely assumed that the intrinsic matter of muscle is not elastic.

The attraction of cohesion, in the parts of muscle, is strongest in the direction of the fibres, it being double that of the contrary, or transverse direction.

When muscles are capable of reiterated contractions and relaxations, they are said to be alive, or to possess irritability. This quality fits the organ for its functions. Irritability will be considered, throughout the present lecture, as a quality only.

When muscles have ceased to be irritable, their cohesive attraction in the direction of their fibres is diminished, but it remains unaltered in the transverse direction.

The hinder limbs of a frog attached to the pelvis being stripped of the skin, one of them was immersed in water at 115° of FAHRENHEIT, during two minutes, when it ceased to be irritable. The thigh bones were broken in the middle, without injuring the muscles, and a scale affixed to the ancle of each limb: a tape passed between the thighs was employed to suspend the apparatus. Weights were gradually introduced into each scale, until, with five pounds avoirdupois, the dead thigh was ruptured across the fleshy bellies of its muscles.

The irritable thigh sustained six pounds weight avoirdupois, and was ruptured in the same manner. This experiment was repeated on other frogs, where one limb had been killed by a watery solution of opium, and on another where essential oil of cherry laurel* was employed: in each experiment, the

* Distilled oil from the leaves of the *Prunus Lauro-cerasus*.

irritable limb sustained a weight one-sixth heavier than the dead limb.

It may be remarked, in confirmation of these experiments, that when muscles act more powerfully, or more rapidly, than is equal to the strength of the sustaining parts, they do not usually rupture their fleshy fibres, but break their tendons, or even an intervening bone, as in the instances of ruptured tendo Achillis, and fractured patella. Instances have however occurred, wherein the fleshy bellies of muscles have been lacerated by spasmodic actions; as in tetanus the recti abdominis have been torn asunder, and the gastrocnemii in cramps; but in those examples it seems that either the antagonists produce the effect, or the over-excited parts tear the less excited in the same muscle. From whence it may be inferred, that the attraction of cohesion in the matter of muscle is considerably greater during the act of contracting, than during the passive state of tone, or irritable quiescence, a fact which has been always assumed by anatomists from the determinate forces which muscles exert.

The muscular parts of different classes of animals vary in colour and texture, and not unfrequently those variations occur in the same individual.

The muscles of fishes and vermes are often colourless, those of the mammalia and birds being always red: the amphibia, the acipenser, and squalus genera, have frequently both red and colourless muscles in the same animal.

Some birds, as the black game,* have the external pectoral muscles of a deep red colour, whilst the internal are pale.

In texture, the fasciculi vary in thickness, and the reticular

* *Tetrao tetrix. Linn.*

membrane is in some parts coarse, and in others delicate: the heart is always compacted together by a delicate reticular membrane, and the external glutæi by a coarser species.

An example of the origin of muscle is presented in the history of the incubated egg, but whether the rudiments of the punctum saliens be part of the cicatricula organised by the parent, or a structure resulting from the first process of incubation, may be doubtful: the little evidence to be obtained on this point seems in favour of the former opinion; a regular confirmation of which would improve the knowledge of animal generation by shewing that it is gemmiferous. There are sufficient analogies of this kind in nature, if reasoning from analogies were proper for the present occasion.

The punctum saliens, during its first actions, is not encompassed by any fibres discoverable with microscopes, and the vascular system is not then evolved, the blood flowing forwards, and backwards, in the same vessels. The commencement of life in animals of complex structure is, from the preceding fact, like the ultimate organization of the simpler classes.

It is obvious that the muscles of birds are formed out of the albumen ovi, the vitellus, and the atmospheric air, acted upon by a certain temperature. The albumen of a bird's egg is wholly consumed during incubation, and the vitellus little diminished, proving that the albumen contains the principal elementary materials of the animal thus generated; and it follows that the muscular parts, which constitute the greater proportion of such animals when hatched, are made out of the albumen, a small portion of the vitellus, and certain elements, or small quantities of the whole compound of the atmosphere.

The muscles of birds are not different, in any respect, from those of quadrupeds of the class of mammalia.

The anatomical structure of muscular fibres is generally complex, as those fibres are connected with membrane, blood-vessels, nerves, and lymphæducts; which seem to be only appendages of convenience to the essential matter of muscle.

A muscular fibre, duly prepared by washing away the adhering extraneous substances, and exposed to view in a powerful microscope, is undoubtedly a solid cylinder, the covering of which is reticular membrane, and the contained part a pulpy substance irregularly granulated, and of little cohesive power when dead.

A difficulty has often subsisted among anatomists concerning the ultimate fibres of muscles; and, because of their tenuity, some persons have considered them infinitely divisible, a position which may be contradicted at any time, by an hour's labour at the microscope.

The arteries arboresce copiously upon the reticular coat of the muscular fibre, and in warm-blooded animals these vessels are of sufficient capacity to admit the red particles of blood, but the intrinsic matter of muscle, contained within the ultimate cylinder, has no red particles.

The arteries of muscles anastomose with corresponding veins; but this course of a continuous canal cannot be supposed to act in a direct manner upon the matter of muscle.

The capillary arteries terminating in the muscular fibre must alone effect all the changes of increase in the bulk, or number, of fibres, in the replenishment of exhausted materials, and in the repair of injuries; some of these necessities may be supposed to be continually operating. It is well known, that

the circulation of the blood is not essential to muscular action; so that the mode of distribution of the blood vessels, and the differences in their size, or number, as applied to muscles, can only be adaptations to some special convenience.

Another prevalent opinion among anatomists, is the infinite extension of vascularity, which is contradicted in a direct manner by comparative researches. The several parts of a quadruped are sensibly more or less vascular, and of different contextures; and, admitting that the varied diameter of the blood vessels disposed in each species of substance, were to be constituted by the gross sensible differences of their larger vessels only, yet, if the ultimate vessels were in all cases equally numerous, then the sole remaining cause of dissimilarity would be in the compacting of the vessels. The vasa vasorum of the larger trunks furnish no reason, excepting that of a loose analogy, for the supposition of vasa vasorum extended without limits. Moreover, the circulating fluids of all animals are composed of water, which gives them fluidity, and of animalised particles of defined configuration and bulk; it follows that the vessels through which such fluids are to pass, must be of sufficient capacity for the size of the particles, and that smaller vessels could only filtrate water devoid of such animal particles: a position repugnant to all the known facts of the circulation of blood, and the animal economy.

The capillary arteries which terminate in the muscular fibre, must be secretory vessels for depositing the muscular matter, the lymphæducts serving to remove the superfluous extravasated watery fluids, and the decayed substances which are unfit for use.

The lymphæducts are not so numerous as the blood vessels,

and certainly do not extend to every muscular fibre: they appear to receive their contained fluids from the intersticial spaces formed by the reticular or cellular membrane, and not from the projecting open ends of tubes, as is generally represented. This mode of receiving fluids out of a cellular structure, and conveying them into cylindrical vessels, is exemplified in the corpora cavernosa, and corpus spongiosum penis, where arterial blood is poured into cellular or reticular cavities, and from thence it passes into common veins by the gradual coarctation of the cellular canals.

In the common green turtle, the lacteal vessels universally arise from the loose cellular membrane, situated between the internal spongy coat of the intestines and the muscular coat. The cellular structure may be filled from the lacteals, or the lacteals from the cellular cavities. When injecting the smaller branches of the lymphæducts retrograde in an œdematous human leg, I saw, very distinctly, three orifices of these vessels terminating in the angles of the cells, into which the quicksilver trickled. The preparation is preserved, and a drawing of the appearance made at the time. It was also proved, by many experiments, that neither the lymphæducts, nor the veins, have any valves in their minute branches.

The nerves of voluntary muscles separate from the same bundles of fibrils with the nerves which are distributed in the skin, and other parts, for sensation; but a greater proportion of nerve is appropriated to the voluntary muscles, than to any other substances, the organs of the senses excepted.

The nerves of volition all arise from the parts formed by the junction of the two great masses of the brain, called the Cerebrum and Cerebellum, and from the extension of that

substance throughout the canal of the vertebræ. Another class of muscles, which are not subject to the will, are supplied by peculiar nerves; they are much smaller, in proportion to the bulk of the parts on which they are distributed, than those of the voluntary muscles; they contain less of the white opaque medullary substance than the other nerves, and unite their fibrils, forming numerous anastomoses with all the other nerves of the body, excepting those appropriated to the organs of the senses. There are enlargements at several of these junctions, called Ganglions, and which are composed of a less proportion of the medullary substance, and their texture is firmer than that of ordinary nerves.

The terminal extremities of nerves have been usually considered of unlimited extension; by accurate dissection however, and the aid of magnifying glasses, the extreme fibrils of nerves are easily traced as far as their sensible properties, and their continuity extends. The fibrils cease to be subdivided whilst perfectly visible to the naked eye, in the voluntary muscles of large animals, and the spaces they occupy upon superficies where they seem to end, leave a remarkable excess of parts unoccupied by those fibrils. The extreme fibrils of nerves lose their opacity, the medullary substance appears soft and transparent, the enveloping membrane becomes pellucid, and the whole fibril is destitute of the tenacity necessary to preserve its own distinctness; it seems to be diffused and mingled with the substances in which it ends. Thus the ultimate terminations of nerves for volition, and ordinary sensation, appear to be in the reticular membrane, the common covering of all the different substances in an animal body, and the connecting medium of all dissimilar parts.

By this simple disposition, the medullary substance of nerve is spread through all organized, sensible, or motive parts, forming a continuity which is probably the occasion of sympathy. Peculiar nerves, such as the first and second pairs, and the portio mollis of the seventh, terminate in an expanse of medullary substance which combines with other parts and membranes, still keeping the sensible excess of the peculiar medullary matter.

The peculiar substance of nerves must in time become inefficient; and, as it is liable to injuries, the powers of restoration, and repair, are extended to that material. The reunion of nerves after their division, and the reproduction after part of a nerve has been cut away, have been established by decisive experiments. Whether there is any new medullary substance employed to fill up the break, and, if so, whether the new substance be generated at the part, or protruded along the nervous theca from the brain, are points undetermined: the history of the formation of a fœtus, the structure of certain monsters, and the organization of simple animals, all seem to favour the probability, that the medullary matter of nerves is formed at the parts where it is required, and not in the principal seat of the cerebral medulla.

This doctrine, clearly established, would lead to the belief of a very extended commixture of this peculiar matter in all the sensible and irritable parts of animals, leaving the nerves in their limited distribution, the simple office of conveying impressions from the two sentient masses with which their extremities are connected. The most simple animals in whom no visible appearances of brain or nerves are to be found, and no fibrous arrangement of muscles, may be considered of this

description: Mr. JOHN HUNTER appeared to have had some incomplete notions upon this subject, which may be gathered from his representation of a materia vitæ in his Treatise on the Blood, &c. Perhaps it would be more proper to distinguish the peculiar matter of muscle by some specific term, such, for example, as materia contractilis.

A particular adaptation for the nerves which supply the electrical batteries of the torpedo, and gymnotus, is observable, on the exit of each from the skull; over which there is a firm cartilage acting as a yoke, with a muscle affixed to it, for the obvious purpose of compression: so that a voluntary muscle probably governs the operations of the battery.

The matter of the nerves, and brain, is very similar in all the different classes of animals.

The external configuration of animals is not more varied than their internal structure.

The bulk of an animal, the limitation of its existence, the medium in which it lives, and the habits it is destined to pursue, are each, and all of them, so many indications of the complexity or simplicity of their internal structure. It is notorious that the number of organs, and of members, is varied in all the different classes of animals; the vascular and nervous systems, the respiratory, and digestive organs, the parts for procreation, and the instruments of motion, are severally varied, and adapted to the condition of the species. This modification of anatomical structure is extended in the lowest tribes of animals, until the body appears to be one homogeneous substance. The cavity for receiving the food is indifferently the internal, or external surface, for they may be inverted, and still continue to digest food; the limbs or tentacula may be cut off,

and they will be regenerated without apparent inconvenience to the individual: the whole animal is equally sensible, equally irritable, equally alive: its procreation is gemmiferous. Every part is pervaded by the nutritious juices, every part is acted upon by the respiratory influence, every part is equally capable of motion, and of altering its figure in all directions, whilst neither blood-vessels, nerves, nor muscular fibres, are discoverable by any of the modes of investigation hitherto instituted.

From this abstract animal (if such a term may be admitted) up to the human frame, the variety of accessory parts, and of organs by which a complicated machinery is operated, exhibit infinite marks of design, and of accommodations to the purposes which fix the order of nature.

In the more complicated animals, there are parts adapted for trivial conveniences, much of their materials not being alive, and the entire offices of some liable to be dispensed with. The water transfused throughout the interstitial spaces of the animal fabric, the combinations with lime in bones, shells, and teeth; the horns, hoofs, spines, hairs, feathers, and cuticular coverings, are all of them, or the principal parts of their substance, extra-vascular, insensible, and unalterable by the animal functions after they are completed. I have formed an opinion, grounded on extensive observation, that many more parts of animal bodies may be considered as inanimate substances; even the reticular membrane itself seems to be of this class, and tendons, which may be the condensed state of it; but these particulars are foreign to the present occasion.

The deduction now to be made, and applied to the history of muscular motion, is, that animated matter may be connected with inanimate; this is exemplified in the adhesions of the

muscles of multi-valve, and bi-valve shell fish, to the inorganic shell, the cancer Bernhardus to the dead shells of other animals, and in the transplantation of teeth. All of which, although somewhat contrary to received opinion, have certainly no degree of vascularity, or vital connection with the inhabitant; these shells being liable to transudations of cupreous salts and other poisonous substances, whilst the animal remains uninjured. A variety of proofs to the same effect might be adduced, but it would be disrespectful to this learned Body to urge any farther illustrations on a subject so obvious.

The effects of subdivision, or comminution of parts among the complicated organized bodies, is unlike that of mineral bodies: in the latter instance, the entire properties of the substance are retained, however extensive the subdivision; in the former substances, the comminution of parts destroys the essential texture and composition, by separating the gross arrangements of structure upon which their specific properties depend. From similar causes it seems to arise, that animals of minute bulk are necessarily of simple structure: size alone is not, however, the sole cause of their simple organization, because examples are sufficiently numerous wherein the animal attains considerable bulk, and is of simple structure, and *vice versâ*: but, in the former, the medium in which they live, and the habits they assume, are such as do not require extensive appendages, whilst the smaller complex animals are destined to more difficult, and more active exertions. It may be assumed however, as an invariable position, that the minutest animals are all of simple organization.

Upon a small scale, life may be carried on with simple materials; but the management, and provisions for bulky animals,

with numerous limbs, and variety of organs, and appendages of convenience, are not effected by simple apparatus; thus, the skeleton which gives a determinate figure to the species, supports its soft parts, and admits of a geometrical motion, is placed interiorly, where the bulk of the animal admits of the bones being sufficiently strong, and yet light enough for the moving powers; but the skeleton is placed externally, where the body is reduced below a certain magnitude, or where the movements of the animal are not to be of the floating kind; in which last case the bulk is not an absolute cause. The examples of testaceous vermes, and coleopterous, as well as most other insects, are universally known.

The opinion of the muscularity of the crystalline lens of the eye, so ingeniously urged by a learned member of this Society, is probably well founded; as the arrangement of radiating lines of the matter of muscle, from the centre to the circumference of the lens, and these compacted into angular masses, would produce specific alterations in its figure.

This rapid sketch of the history of muscular structure has been obtruded before the Royal Society to introduce the principal experiments, and reasonings which are to follow: they are not ordered with so much exactness as becomes a more deliberate essay, but the intention already stated, and the limits of a lecture are offered as the apology.

Temperature has an essential influence over the actions of muscles, but it is not necessary that the same temperature should subsist in all muscles during their actions; neither is it essential that all the muscular parts of the same animal should be of uniform temperatures for the due performance of the motive functions.

It appears that all the classes of animals are endowed with some power of producing thermometrical heat, since it has been so established in the amphibia, pisces, vermes, and insecta, by Mr. JOHN HUNTER; a fact which has been verified to my own experience; the term "cold-blooded" is therefore only relative. The ratio of this power is not, however, in these examples, sufficient to preserve their equable temperature in cold climates, so that they yield to the changes of the atmosphere, or the medium in which they reside, and most of them become torpid, approaching to the degree of freezing water. Even the mammalia, and aves, possess only a power of resisting certain limited degrees of cold; and their surfaces, as well as their limbs, being distant from the heart, and principal blood-vessels, the muscular parts so situated are subject to considerable variations in their temperature, the influence of which is known.

In those classes of animals which have little power of generating heat, there are remarkable differences in the structure of their lungs, and in the composition of their blood, from the mammalia and aves.

Respiration is one of the known causes which influences the temperatures of animals: where these organs are extensive, the respirations are performed at regular intervals, and are not governed by the will, the whole mass of blood being exposed to the atmosphere in each circulation. In all such animals living without the tropics, their temperature ranges above the ordinary heat of the atmosphere, their blood contains more of the red particles than in the other classes, and their muscular irritability ceases more rapidly after violent death.

The respirations of the animals denominated "cold-blooded,"

are effected differently from those of high temperature; in some of them, as the amphibia of LINNÆUS, the lungs receive atmospheric air, which is arbitrarily retained in large cells, and not alternately, and frequently changed. The fishes, and the testaceous vermes, have lungs which expose their blood to water, but whether the water alone, or the atmospheric air mingled with it, furnish the changes in the pulmonary blood, is not known.

In most of the genera of insects, the lungs are arborescent tubes containing air, which, by these channels, is carried to every vascular part of the body. Some of the vermes of the simpler construction have no appearance of distinct organs, but the respiratory influence is nevertheless essential to their existence, and it seems to be effected on the surface of the whole body.

In all the colder animals, the blood contains a smaller proportion of the red colouring particles than in the mammalia, and aves; the red blood is limited to certain portions of the body, and many animals have none of the red particles.

The following animals were put into separate glass vessels, each filled with a pound weight of distilled water, previously boiled to expel the air, and the vessels inverted into quicksilver: *viz.* one gold fish, one frog, two leeches, and one fresh-water muscle.* These animals were confined for several days, and exposed to the sun in the day time, during the month of January, the temperature being from 42° to 48°, but no air bubbles were produced in the vessels, nor any sensible diminution of the water. The frog died on the third day, the fish on the fifth, the leeches on the eighth, and the fresh-water

* *Mytilus dentinus.*

muscle on the thirteenth. This unsuccessful experiment was made with the hope of ascertaining the changes produced in water by the respiration of aquatic animals, but the water had not undergone any chemical alteration.

Animals of the class mammalia which hybernate, and become torpid in the winter, have at all times a power of subsisting under a confined respiration, which would destroy other animals not having this peculiar habit. In all the hybernating mammalia there is a peculiar structure of the heart, and its principal veins; the superior cava divides into two trunks; the left, passing over the left auricle of the heart, opens into the inferior part of the right auricle, near to the entrance of the vena cava inferior. The veins usually called azygos, accumulate into two trunks, which open each into the branch of the vena cava superior, on its own side of the thorax. The intercostal arteries and veins in these animals are unusually large.

This tribe of quadrupeds have the habit of rolling up their bodies into the form of a ball during ordinary sleep, and they invariably assume the same attitude when in the torpid state: the limbs are all folded into the hollow made by the bending of the body; the clavicles, or first ribs, and the sternum, are pressed against the fore part of the neck, so as to interrupt the flow of blood which supplies the head, and to compress the trachea: the abdominal viscera, and the hinder limbs are pushed against the diaphragm, so as to interrupt its motions, and to impede the flow of blood through the large vessels which penetrate it, and the longitudinal extension of the cavity of the thorax is entirely obstructed. Thus a confined circulation of the blood is carried on through the heart, probably

adapted to the last weak actions of life, and to its gradual recommencement.

This diminished respiration is the first step into the state of torpidity; a deep sleep accompanies it; respiration then ceases altogether; the animal temperature is totally destroyed, coldness and insensibility take place, and finally the heart concludes its motions, and the muscles cease to be irritable. It is worthy of remark that a confined air, and a confined respiration, ever precede these phenomena: the animal retires from the open atmosphere, his mouth and nostrils are brought into contact with his chest, and enveloped in fur; the limbs become rigid, but the blood never coagulates during the dormant state. On being roused, the animal yawns, the respirations are fluttering, the heart acts slowly and irregularly, he begins to stretch out his limbs, and proceeds in quest of food. During this dormancy, the animal may be frozen, without the destruction of the muscular irritability, and this always happens to the garden snail,* and to the chrysalides of many insects during the winter of this climate.

The loss of motion and sensation from the influence of low temperature, accompany each other, and the capillaries of the vascular system appear to become contracted by the loss of animal heat, as in the examples of numbness from cold. Whether the cessation of muscular action be owing to the impeded influence of the nerves, or to the lowered temperature of the muscles themselves, is doubtful; but the known influence of cold upon the sensorial system, rather favours the supposition that a certain temperature is necessary for the transmission of nervous influence, as well as sensation.

* *Helix nemoralis.*

The hybernating animals require a longer time in drowning than others. A full grown hedge-hog was submersed in water at 48°, and firmly retained there; air-bubbles began instantly to ascend, and continued during four minutes; the animal was not yet anxious for its liberty. After seven minutes it began to look about, attempting to escape; at ten minutes it rolled itself up, only protruding the snout, which was hastily retracted on being touched with the finger, and even the approach of the finger caused it to retract. After fifteen minutes complete submersion, the animal still remained rolled up, and withdrew its nose on being touched. After remaining thirty minutes under water, the animal was laid upon flannel, in an atmosphere of 82°, with its head inclined downwards; it soon began to relax the sphincter muscle which contracts the skin, slow respirations commenced, and it recovered entirely, without artificial aid, after two hours. Another hedge-hog submersed in water at 94°, remained quiet until after five minutes; about the eighth minute it stretched itself out, and expired at the tenth. It remained relaxed, and extended, after the cessation of the vital functions; and its muscles were relaxed, contrary to those of the animal drowned in the colder water.

The irritability of the heart is inseparably connected with respiration. Whenever the inhaled gas differs in its properties from the common atmosphere, the muscular and sensible parts of the system exhibit the change: the actions of the heart are altered or suspended, and the whole muscular and sensorial systems partake of the disorder: the temperature of animals, as before intimated, seems altogether dependant on the respiratory functions, although it still remains uncertain in what manner this is effected.

The blood appears to be the medium of conveying heat to the different parts of the body; and the changes of animal temperature in the same individual at various times, or in its several parts, are always connected with the degree of rapidity of the circulation. It is no very wide stretch of physiological deduction to infer, that this increased temperature is produced by the more frequent exposure of the mass of blood to the respiratory influence, and the short time allowed in each circuit for the loss of the acquired heat.

The blood of an animal is usually coagulated immediately after death, and the muscles are contracted; but, in some peculiar modes of death, neither the one, nor the other of these effects are produced: with such exceptions, the two phenomena are concomitant.

A preternatural increase of animal heat delays the coagulation of the blood, and the last contractions of the muscles: these contractions gradually disappear, before any changes from putrefaction are manifested; but the cup in the coagulum of blood does not relax in the same manner; hence it may be inferred, that the final contraction of muscles is not the coagulation of the blood contained in them; neither is it a change in the reticular membrane, nor in the blood-vessels, because such contractions are not general throughout those substances. The coagulation of the blood is a certain criterion of death. The reiterated visitations of blood are not essential to muscular irritability, because the limbs of animals, separated from the body, continue for a long time afterwards capable of contractions, and relaxations.

The constituent elementary materials of which the peculiar animal and vegetable substances consist, are not separable by

any chemical processes hitherto instituted, in such manner as to allow of a recombination into their former state. The composition of these substances appears to be naturally of transient duration, and the attractions of the elementary materials which form the gross substances are so loose and unsettled, that they are all decomposed without the intervention of any agent, merely by the operation of their own elementary parts on each other.

An extensive discussion of the chemical properties attaching to the matter of muscle would be a labour unsuited to this occasion; I should not, however, discharge my present duty, if I omitted to say, that all such investigations can only be profitable when effected by simple processes, and when made upon the raw materials of the animal fabric, such, perhaps, as the albumen of eggs, and the blood. But, until by synthetical experiments the peculiar substances of animals are composed from what are considered to be elementary materials, or the changes of organic secretion imitated by art, it cannot be hoped that any determinate knowledge should be established upon which the physiology of muscles may be explained. Such researches and investigations promise, however, the most probable ultimate success, since the phenomena are nearest allied to those of chemistry, and since all other hypotheses have, in their turns, proved unsatisfactory.

*Facts and Experiments tending to support and illustrate the preceding Argument.*

An emaciated horse was killed by dividing the medulla spinalis, and the large blood-vessels under the first bone of the sternum.

The temperature of the flowing blood was as follows:

| | |
|---|---|
| Spleen | 103 |
| Stomach | 101 |
| Colon | 98 |
| Bladder of urine | 98 |
| Atmosphere | 30 |

Three pigs, killed by a blow on the head, and by the immediate division of the large arteries and veins, entering the middle of the basis of the heart, had the blood flowing from these vessels of 106, 106½, and 107½; the atmospheric temperature being at 31°.

An ox, killed in a similar manner, the blood 105°; atmosphere 50°.

Three sheep, killed by dividing the carotid arteries, and internal jugular veins: their blood 105, 105, 105½°; atmosphere 41°.

Three frogs, kept for many days in an equable atmosphere at 54°; their stomachs 64°.

The watery fluid issuing from a person tapped for dropsy of the belly 101°; the atmosphere being 43°, and the temperature of the superficies of the body at 96°.

These temperatures are considerably higher than the common estimation.

A man's arm being introduced within a glass cylinder, it was duly closed at the end which embraced the head of the humerus; the vessel being inverted, water at 97° was poured in, so as to fill it. A ground brass plate closed the lower aperture, and a barometer tube communicated with the water at the bottom of the cylinder. This apparatus including the arm, was again inverted, so that the barometer tube became a

gage, and no air was suffered to remain in the apparatus. On the slightest action with the muscles of the hand, or fore-arm, the water ascended rapidly in the gage, making librations of six and eight inches length in the barometer tube, on each contraction and relaxation of the muscles.

The remarkable effects of crimping fish by immersion in water, after the usual signs of life have disappeared, are worthy attention; and whenever the rigid contractions of death have not taken place, this process may be practised with success. The sea fish designed for crimping life usually struck on the head when caught, which, it is said, protracts the term of this capability; and the muscles which retain this property longest are those about the head. Many transverse sections of the muscles being made, and the fish immersed in cold water, the contractions called crimping take place in about five minutes; but, if the mass be large, it often requires thirty minutes to complete the process.

Two flounders, each weighing 1926 grains, the one being in a state for crimping, the other dead and rigid, were put into water at 48°, each being equally scored with a knife. After half an hour, the crimped fish had gained in weight 59 grains, but the dead fish had lost 7 grains. The specific gravity of the crimped fish was greater than that of the dead fish, but a quantity of air-bubbles adhered to the surfaces of the crimped muscles, which were rubbed off before weighing; this gas was not inflammable.

The specific gravity of the crimped fish – – 1,105
of the dead fish, after an equal immersion in water – 1,090.

So that the accession of water specifically lighter than the

muscle of fish, did not diminish the specific gravity of crimped muscle, but the contrary: a proof that condensation had taken place.

A piece of cod-fish weighing twelve pounds, gained in weight, by crimping, two ounces avoirdupois; and another less vivacious piece, of fifteen pounds, gained one ounce and half.*

The hinder limb of a frog, having the skin stripped off, and weighing 77$\frac{1}{10}$ grains, was immersed in water at 54°, and suffered to remain nineteen hours, when it had become rigid, and weighed 100$\frac{1}{2}$ grains. The specific gravity of the contracted limb had increased, as in the crimped fish.

Six hundred and thirty grains weight of the subscapularis muscle of a calf, which had been killed two days from the 16th of January, was immersed in New River water at 45°. After ninety minutes, the muscle was contracted, and weighed in air 770 grains: it had also increased in specific gravity, but the quantity of air-bubbles formed in the interstitial spaces of the reticular membrane made it difficult to ascertain the degree.

Some of the smallest fasciculi of muscular fibres from the same veal, which had not been immersed in water, were placed on a glass plate, in the field of a powerful microscope, and a drop of water thrown over them, at the temperature of 54°, the atmosphere in the room being 57°. They instantly began to contract, and became tortuous.

On confining the ends of another fibril with little weights of glass, it contracted two-thirds of its former length, by similar

* I am informed that the crimping of fresh water fishes requires hard water, or such as does not suit the purposes of washing with soap. This fact is substantiated by the practice of the London fishmongers, whose experience has taught them to employ pump water, or what is commonly called hard water.

treatment. The same experiment was made on the muscular fibres of lamb and beef, twelve hours after the animals had been killed, with the like results. Neither vinegar, nor water saturated with muriate of soda, nor strong ardent spirit, nor olive oil, had any such effect upon the muscular fibres.

The amphibia, and coleopterous insects, become torpid at 34°. At 36° they move slowly, and with difficulty; and, at a lower temperature their muscles cease to be irritable. The muscles of warm-blooded animals are similarly affected by cold.

The hinder limbs of a frog were skinned and exposed to cold at 30°, and the muscles were kept frozen for eight hours, but on thawing them, they were perfectly irritable.

The same process was employed in the temperature of 20°, and the muscles kept frozen for twelve hours, but that did not destroy the irritability.

In the heat of 100°, the muscles of cold-blooded animals fall into the contractions of death; and at 110°, all those of warm blood, as far as these experiments have been extended. The muscles of warm-blooded animals, which always contain more red particles in their substance than those of cold blood, are sooner deprived of their irritability, even although their relative temperatures are preserved; and respiration in the former tribe is more essential to life than in the latter.

Many substances accelerate the cessation of irritability in muscles when applied to their naked fibrils, such as all the narcotic vegetable poisons, muriate of soda, and the bile of animals; but they do not produce any other apparent change in muscles, than that of the last contraction. Discharges of electricity passed through muscles, destroy their irritability, but leave them apparently inflated with small bubbles of gas;

perhaps some combination obtains which decomposes the water.

The four separated limbs of a recent frog were skinned, and immersed in different fluids; *viz.* No. 1, in a phial containing six ounces by measure of a saturated aqueous solution of liver of sulphur made with potash; No. 2, in a diluted acetic acid, consisting of one drachm of concentrated acid to six of water; No. 3, in a diluted alkali, composed of caustic vegetable alkali one drachm, of water six ounces; No. 4, in pure distilled water.

The phials were all corked, and the temperature of their contents was 46°.

The limb contained in the phial No. 1, after remaining twenty minutes, had acquired a pale red colour, and the muscles were highly irritable.

The limb in No. 2, after the same duration, had become rigid, white, and swollen; it was not at all irritable. By removing the limb into a diluted solution of vegetable alkali, the muscles were relaxed, but no signs of irritability returned.

No. 3, under all the former circumstances, retained its previous appearances, and was irritable, but less so than No. 1.

No. 4 had become rigid, and the final contraction had taken place.

Other causes of the loss of muscular irritability occur in pathological testimonies, some examples of which may not be ineligible for the present subject. Workmen whose hands are unavoidably exposed to the contact of white lead, are liable to what is called a palsy in the hands and wrists, from a torpidity of the muscles of the fore arm. This affection seems to be decidedly local, because, in many instances, neither the brain, nor the other members, partake of the disorder; and it oftenest

affects the right hand. An ingenious practical chemist in London has frequently experienced spasms and rigidity in the muscles of his fore arms, from affusions of nitric acid over the cuticle of the hand and arm. The use of mercury occasionally brings on a similar rigidity in the masseter muscles.

A smaller quantity of blood flows through a muscle during the state of contraction, than during the quiescent state, as is evinced by the pale colour of red muscles when contracted. The retardation of the flow of blood from the veins of the fore arm, during venæsection, when the muscles of the limb are kept rigid, and the increased flow after alternate relaxations, induces the probability, that a temporary retardation of the blood in the muscular fibrils takes place during each contraction, and that its free course obtains again during the relaxation. This state of the vascular system in a contracted muscle, does not, however, explain the diminution of its bulk, although it may have some influence on the limb of a living animal.

When muscles are vigorously contracted, their sensibility to pain is nearly destroyed; this means is employed by jugglers for the purpose of suffering pins to be thrust into the calf of the leg, and other muscular parts with impunity: it is indeed reasonable to expect, *a priori*, that the sensation, and the voluntary influence, cannot pass along the nerves at the same time.

In addition to the influences already enumerated, the human muscles are susceptible of changes from extraordinary occurrences of sensible impressions. Long continued attention to interesting visible objects, or to audible sensations, are known to exhaust the muscular strength: intense thought and anxiety, weaken the muscular powers, and the passions of grief and

fear produce the same effect suddenly: whilst the contrary feelings, such as the prospect of immediate enjoyment, or moderate hilarity, give more than ordinary vigour.

It is a very remarkable fact in the history of animal nature, that the mental operations may become almost automatic, and, under such habit, be kept in action, without any interval of rest, far beyond the time which the ordinary state of health permits, as in the examples of certain maniacs, who are enabled without any inconvenience, to exert both mind and body for many days incessantly. The habits of particular modes of labour and exercise are soon acquired, after which, the actions become automatic, demand little attention, cease to be irksome, and are effected with little fatigue: by this happy provision of nature, the habit of industry becomes a source of pleasure, and the same appears to be extended to the docile animals which co-operate with man in his labours.

Three classes of muscles are found in the more complicated animals. Those which are constantly governed by the will, or directing power of the mind, are called voluntary muscles. Another class, which operate without the consciousness of the mind, are denominated involuntary; and a mixed kind occur in the example of respiratory muscles, which are governed by the will to a limited extent; nevertheless the exigencies of the animal feelings eventually urge the respiratory movements in despight of the will. These last muscles appear to have become automatic by the continuance of habit.

The uses of voluntary muscles are attained by experience, imitation, and instruction: but some of them are never called into action among Europeans, as the muscles of the external ears, and generally the occipito-frontalis. The purely involu-

luntary muscles are each acted upon by different substances, which appear to be their peculiar stimuli; and these stimuli co-operate with the sensorial influence in producing their contractions: for example, the bile appears to be the appropriate stimulus of the muscular fibres of the alimentary canal below the stomach, because the absence of it renders those passages torpid. The digested aliment, or perhaps the gastric juice in a certain state, excites the stomach. The blood stimulates the heart, light the iris of the eye, and mechanical pressure seems to excite the muscles of the œsophagus. The last cause may perhaps be illustrated by the instances of compression upon the voluntary muscles, when partially contracted, of which there are many familiar examples. Probably the muscles of the ossicula auditûs are awakened by the tremors of sound; and this may be the occasion of the peculiar arrangement observable in the chorda tympani, which serves those muscles.

These extraneous stimuli seem only to act in conjunction with the sensorial power, derived by those muscles from the gangliated nerves, because the passions of the mind alter the muscular actions of the heart, the alimentary canal, the respiratory muscles, and the iris; so that probably the respective stimuli already enumerated, only act subserviently, by awakening the attention of the sensorial power, (if that expression may be allowed,) and thereby calling forth the nervous influence, which, from the peculiar organization of the great chain of sympathetic nerves, is effected without consciousness: for, when the attention of the mind, or the more interesting passions prevail, all the involuntary muscles act irregularly, and unsteadily, or wholly cease. The movements of the iris of the common parrot is a striking example of the mixed influence.

The muscles of the lower tribes of animals, which are often

entirely supplied by nerves coming from ganglions, appear of this class; and thus the animal motions are principally regulated by the external stimuli, of which the occurrence seems to agree with the animal necessities: but the extensive illustrations which comparative anatomy affords on this point, are much too copious for any detail in this place.

There are two states of muscles, one active, which is that of contraction, the other, a state of ordinary tone, or relaxation, which may be considered passive, as far as it relates to the mind; but the sensorial or nervous power seems never to be quiescent, as it respects either the voluntary or involuntary muscles during life. The yielding of the sphincters appears to depend on their being overpowered by antagonist muscles, rather than on voluntary relaxation, as is commonly supposed.

I have now finished this endeavour to exhibit the more recent historical facts connected with muscular motion.

It will be obvious to every one, that much remains to be done, before any adequate theory can be proposed. I have borrowed from the labours of others, without acknowledgement, because it would be tedious to trace every fact, and every opinion to its proper authority: many of the views are perhaps peculiar to myself, and I have adduced many general assumptions and conclusions, without offering the particular evidence for their confirmation, from a desire to keep in view the remembrance of retrospective accounts, and to combine them with intimations for future research. The due cultivation of this interesting pursuit cannot fail to elucidate many of the phenomena in question, to remove premature and ill founded physiological opinions, and eventually to aid in rendering the medical art more beneficial, by establishing its doctrines on more extensive and accurate views of the animal economy.

II. *Experiments for ascertaining how far Telescopes will enable us to determine very small Angles, and to distinguish the real from the spurious Diameters of celestial and terrestrial Objects: with an Application of the Result of these Experiments to a Series of Observations on the Nature and Magnitude of Mr.* Harding's *lately discovered Star. By* William Herschel, *LL.D. F.R.S.*

Read December 6, 1804.

The discovery of Mr. Harding having added a moving celestial body to the list of those that were known before, I was desirous of ascertaining its magnitude; and as in the observations which it was necessary to make I intended chiefly to use a ten-feet reflector, it appeared to me a desideratum highly worthy of investigation to determine how small a diameter of an object might be seen by this instrument. We know that a very thin line may be perceived, and that objects may be seen when they subtend a very small angle; but the case I wanted to determine relates to a visible disk, a round, well defined appearance, which we may without hesitation affirm to be circular, if not spherical.

In April of the year 1774, I determined a similar question relating to the natural eye: and found that a square area could not be distinguished from an equal circular one till the diameter of the latter came to subtend an angle of 2′ 17″. I did not think it right to apply the same conclusions to a telescopic view

of an object, and therefore had recourse to the following experiments.

*1st Experiment, with the Heads of Pins.*

I selected a set of pins with round heads, and deprived them of their polish by tarnishing them in the flame of a candle. The diameters of the heads were measured by a microscopic projection, with a magnifying power of 80. These measures are so exact, that when repeated they will seldom differ more than a few ten thousandths parts of an inch from each other. Their sizes were as follows: ,1375 ,0863 ,0821 ,0602 ,0425. I placed the pins in a regular order upon a small post erected in my garden, at 2407,85 inches from the centre of the object mirror of my ten-feet reflecting telescope. The focal length of the mirror on Arcturus is 119,64 inches, but on these objects 125,9. The distance was measured with deal rods.

When I looked at these objects in the telescope, I found immediately that only the smallest of them, at this distance could be of any use; for with an eye-glass of 4 inches, which gives the telescope a magnifying power of no more than 31,5, this pin's head appeared to be a round body, and the view left no doubt upon the subject. It subtended an angle of 3",64 at the centre of the mirror, and the magnified angle under which I saw it was 1' 54",6. This low power however required great attention.

With a lens 3,3, power 38,15, I saw it instantly round and globular. The magnified angle was 2' 18",9.

With a magnifying power of 231,8,* I saw it so plainly that

* The powers have been strictly ascertained as they are at the distance where these objects were viewed.

the little notch in the pin's head between the coils of the wire making the head, appeared like a narrow black belt surrounding the pin in the manner of the belts of Jupiter. This notch by the microscopic projection measured ,00475 inch; and subtended an angle, at the centre of the mirror, of 0″,407.

With 309,5 I saw the belt still better, and could follow it easily in its contour.

With 432,0 I could see down into the notch, and saw it well defined within.

With 522,9 the pin's head was a very striking globular object, whose diameter might easily be divided by estimation into ten parts, each of which would be equal to 0″,364.

With 925,6 I saw all the same phenomena still plainer.

The result of this experiment is, that an object having a diameter ,0425 may be easily seen in my telescope to be a round body, when the magnified angle under which it appears is 1′ 18″,9, and that with a high power a part of it, subtending an angle of 0″,364 may be conveniently perceived.

When I considered the purpose of this experiment, I found the result not sufficient to answer my intention; for as the size of the object I viewed obliged me to use a low power, a doubt arose whether the instrument would be equally distinct when a higher should be required. To resolve this question, it was necessary either to remove my objects to a greater distance, or to make them smaller.

*2d Experiment, with small Globules of Sealing-wax.*

I melted some sealing-wax thinly spread on a broad knife, and dipt the point of a fine needle, a little heated, into it, which took up a small globule. With some practice I soon acquired

the art of making them perfectly round and extremely small. To prevent my seeing them at a distance in a different aspect from that in which they were measured under the microscope, I fixed the needles with sealing-wax on small slips of cards before the measures were taken.

Eight of these globules of the following dimensions ,0466 ,0325 ,0290 ,02194 ,0210 ,0169 ,0144 ,00763 were placed upon the post in my garden, and I viewed them in the telescope.

With a power of 231,8 I saw all the first seven numbers well defined, and round, and could see their gradual decrease very precisely from No. 1 to No. 7.

With 303,5 I saw them better, and had a glimpse of No. 8, but could not be sure that I saw it distinctly round; though the magnified angle was 3′ 18″,2.

With 432,0 they are all very palpable objects, and, as a solid body, No. 8 may be seen without difficulty: at the centre of the mirror it subtends an angle of 0″,653. With attention we may also be sure of its roundness; but here the magnified angle is not less than 4′ 42″,1.

With 522,3 I see them all in great perfection as spherical bodies, and the magnitude of No. 7 may be estimated in quarters of its diameters. The angle is 1″,238, and one quarter of it is 0″,313. No. 8 may be divided into two halves with ease; each of which is 0″,327.

With 925,4 I saw No. 8 still better; but sealing-wax is not bright enough for so high a power.

By this experiment it appears, that with a globule so small as ,00763 of a substance not reflecting much light, the magnified angle must be between 4 and 5 minutes before we can see it round. But it also appears that a telescope with a sufficient

power, will show the disk of a faint object when the angle it subtends at the naked eye is no more than 0″,653.

*3d Experiment, with Globules of Silver.*

As the objects made of sealing-wax, on account of their colour, did not appear to be fairly selected for these investigations, I made a set of silver ones. They were formed by running the end of silver wires, the 805th and 840th part of an inch in diameter, into the flame of a candle. It requires some practice to get them globular, as they are very apt to assume the shape of a pear; but they are so easily made that we have only to reject those which do not succeed.

Thirteen of them, in a pretty regular succession of magnitude, were selected and placed upon the post. Their dimensions were ,03956 ,0371 ,0329 ,0317 ,0272 ,0260 ,0187 ,0178 ,0164 ,0123 ,01137 ,00800 ,00556.

For the sake of more conveniency I had removed my telescope from its station in the library to a work-room. The distance of the objects from the mirror of the telescope, measured with deal rods, was here only 2370.5 inches; and the focal length of the mirror, the magnifying powers of the telescope, and the angles subtended by the objects have been calculated accordingly.

With 522,7 I see all the globules, from No. 1 to No. 13, perfectly well, and can estimate the latter in quarters of its diameter. The angle it subtends at the centre of the mirror is 0″,484; and one quarter of it is 0″,121.

With the same power I see the wires which hold the balls, so well that even the smallest of them may be divided into

half its thickness. It measures ,00237; the angle is 0″,806; and half of it 0″,103.

With 433,0 I see all the globules of a round form, and can by estimation divide No. 13 into two halves. The magnified angle is here 3′ 29″,0, but as its diameter could by estimation be divided into two parts, the round form of a globule somewhat less might probably have been perceived, so that the magnified angle would perhaps not have much exceeded the quantity 2′ 18″,9 that has been assigned before.

After some time the weather became much overcast, and as the globules were placed over a cut hedge, the leaves and interstices of which did not reflect much light, they received the greatest part of their illumination from above. This made them gradually assume the shape of half moons placed horizontally. The dark part of these little lunes, however, did not appear sensibly less than the enlightened part, so that there could not be any thing spurious about them.

By this experiment we find that the telescope acts very well with a high power, and will show an object subtending only 0″,484 so large that we may divide it into quarters of its diameter.

*4th Experiment, with Globules of Pitch, Bee's-wax, and Brimstone.*

I had before objected to sealing-wax globules on account of their dingy-red colour; in the last experiment a doubt was raised with regard to the silver ones, because they were perhaps too glossy. In order to compare the effect of different substances together in the same atmosphere, I put up three

globules, No. 1 of silver, diameter ,011137; No. 2 of sealing-wax ,01125; No. 3 of pitch ,00655.

With 522,7 I saw No. 1 round, and could estimate $\frac{1}{4}$ of its diameter. The angle is 0″,989; $\frac{1}{4}$ of it is 0″,247.

I saw No. 2 round, but of a dusky-red colour. It is not nearly so bright as No. 1; nor does it appear quite so large as the proportional measure of the globules would require. I can estimate $\frac{1}{4}$ of its diameter. The angle is 0″,979; and $\frac{1}{4}$ of it is 0″,826.

No 3 reflects so little light that I can barely perceive the globule, but not its form; and yet it subtends an angle of 0″,568.

To discover whether this ought to be ascribed intirely to the want of reflection of the pitch, I took up some white melted bee's-wax, by dipping the fine point of a needle perpendicularly into it. This happened to be only half a globule, and its diameter was ,0105.

When I examined the object with 523 I saw it with great ease, and could estimate $\frac{1}{4}$ of its diameter. The angle is 0″,914; and $\frac{1}{4}$ of it is 0″,928. I saw also that it was but half a globule.

I took up another, that I might have a round one; but found that again I had only half a globule. It was so perfectly bisected, that art and care united could not have done it better. Its diameter was ,0108. In the telescope I saw its semiglobular form, and could estimate $\frac{1}{4}$ of its diameter.

By some further trials it appeared, that a perfect globule of this substance could not be taken up, the reason of which it is not difficult to perceive; for as it melts with very little heat, it will cool the moment the needle is lifted up; and the surface, which cools first, will be flat.

The roundness of the objects being a material circumstance, I melted a small quantity of the powder of brimstone, and dipping the point of a needle into it, I found that globules, perfectly spherical and extremely small, might be taken up. I had one of them that did not exceed the 640th part of an inch in diameter.

When four of the following sizes, ,00962 ,00912 5 ,00475 ,00237 5 were placed on the post in the garden and viewed from the work-room station with 522,7 I saw No. 1, 2, and 3, round, but No. 4 was invisible.

These globules reflect but little light, so that they are not easily to be distinguished from the surrounding illumination of the atmosphere; but when I placed some dark blue paper a few inches behind them, I then could also perceive No. 4 as a round body. The angle it subtends is 0",207.

*5th Experiment with Objects at a greater Distance.*

Having carried the minuteness of the globules as far as appeared to be proper, I considered that a valuable advantage would be gained by increasing the distance of the objects. The experiments might here be made upon a larger scale, and the body of air through which it would be necessary to view the globules would bring the action of the telescope more upon a par with an application of it to celestial objects.

On a tree, at 9620,4 inches from the object mirror of the telescope, I fixed the sealing-wax globules of the 2d experiment. The distance was measured by a chain compared with deal rods, and by calculation the altitude of the objects has been properly taken into the account.

With 502,6 No. 1 is a very large object; so that were I to

see a celestial body under the same angle, I could never mistake it for a small star. The angle it subtends is 0″,999.

I see the diameters of No. 2 and 3 very clearly, and can divide them by estimation into two parts. half of No. 3 is 0″,311.

I see No. 4 and 5 as round bodies, but cannot divide them by estimation. The diameter of No. 5 is 0″,45. No. 6 may also be seen, but 7 and 8 are invisible.

These objects reflecting too little light, the silver globules of the 3d experiment were placed on the tree. It will be right to mention that they were all so far tarnished by having been out in the open air for more than a fortnight, that no improper reflection was to be apprehended.

The air being uncommonly clear, I saw with 502,6 the globules No. 1, 2, 3, 4, 5, and 6, as well defined black balls. I could easily distinguish $\frac{1}{4}$ of the diameter of No. 6; which is 0″,139.

With 415,7 I saw them all round as far as No. 10 included.

With 502,6 I saw No. 9 and 10 very sharp and black, and could divide No. 10 into two parts, each of which would be 0″,134.

With a new lens, power 759,7, I saw No. 10 better than with 502,6, and could with ease distinguish it into halves, or even third parts of its diameter. $\frac{1}{4}$ of it is 0″,089.

With 223,1 I saw them all as far as No. 10 included as visible objects, but the smallest of them were mere points. No. 6 might be divided with this power into two parts; each being 0″,279.

With 292,1 I saw No. 10 sharp and round. The magnified angle is only 1′ 18″,3. One half of No. 6 may be perceived with great ease.

The weather being as favourable as possible, I saw with 415,7 the globule No. 10 round at first sight; the magnified angle is 1′ 51″,2. I can see No. 12 steadily round; the angle is 0″,172. It is however a mere point, and divisions of it cannot be made.

With a new 10-feet reflector, power 540, the globule No. 10 is beautifully well defined, and $\frac{1}{4}$ of it may be estimated; the angle is 0″,268; $\frac{1}{4}$ of it is 0″,134.

With the old reflector, and 502,6, I see No. 12 steadily round. No. 7, 11, and 13, have met with an accident, and could not be observed.

*6th Experiment with illuminated Globules.*

The night being very dark, 8 silver globules, from ,0291 to ,00596 in diameter were placed on the post, and illuminated by a lantern held up against them.

With 522,7 I saw them all perfectly well, but the small quantity of light thrown on them was not sufficient to make angular experiments upon them. As objects I saw them as easily as in the day time. Probably the phases of the illuminated disks I saw might be such as the moon would show when about 9 or 10 days old. The angle of No. 8, had it been full, would have been 0″,519. A better way of illumination might be contrived.

## SPURIOUS DIAMETERS OF CELESTIAL OBJECTS.

*Observations and Experiments, with Remarks.*

July 17, 1779. With a 7-feet reflector, power 280, I saw the body of Arcturus, very round and well defined. I saw

also ζ Ursæ majoris and other stars equally round, and as well defined.

REMARKS.

(1.) As these diameters are undoubtedly spurious, it follows that, with the stars, the spurious diameters are larger than the real ones, which are too small to be seen.

Sept. 9, 1779. The two stars of ι Bootis are of unequal diameters; one of them being about three times as large as the other.

(2.) From this and many estimations of the spurious diameters of the stars* it follows, not only that they are of different sizes, but also that under the same circumstances, their dimensions are of a permanent nature.

August 25, 1780. The large star of γ Andromedæ is of a very fine reddish colour, and the small one blue.

(3.) By this and many other observations it appears, that the spurious diameters of the stars are differently coloured, and that these colours are permanent when circumstances are the same.

Nov. 23, 1779. I viewed α Geminorum with a power of 449, and saw the two stars in the utmost perfection. The vacancy between them was about 1½ diameter of the largest. I found when I looked with a lower power, that the proportion between the distance and magnitude of the stars underwent an alteration. With 222, the vacancy was 1¼ diameter, and with 112, it was no more than 1 diameter of the smallest of the two stars, or less.

(4.) By many observations, a number of instances of which

* See Catalogues of double Stars. Phil. Trans. for 1782, p. 115; and for 1785, page 40.

may be seen in my catalogues of double stars, their spurious diameters are lessened by increasing the magnifying power, and increase when the power is lowered.

(5.) It is also proved by the same observations, that the increase and decrease of the spurious diameters, is not inversely as the increase and decrease of the magnifying power, but in a much less ratio.

Nov. 13, 1782. The two stars of the double star 40 Lyncis, with a power of 460 are very unequal; and with 227 they are extremely unequal.

(6.) From this we find, that the magnifying power acts unequally on spurious diameters of different magnitudes; less on the large diameters, and more on the small ones.

Aug. 20, 1781. I saw ι Bootis with 460, and the vacancy between the two stars was $1\frac{1}{4}$ diameter of the large one. I then reduced the aperture of the telescope by a circle of pasteboard from 6,8 inches to 3,5, and the vacancy between the two stars became only $\frac{1}{2}$ diameter of the small star.

The proportion of the diameters of the two stars to each other was also changed considerably; for the small one was now at least $\frac{2}{3}$ if not $\frac{3}{4}$ of the large one.

(7.) This shows that when the aperture of the telescope is lessened, it will occasion an increase of the spurious diameters, and when increased will reduce them.

(8.) It also shows that the increase and decrease of the unequal spurious diameters, by an alteration of the aperture of the telescope, is not proportional to the diameters of the stars:

(9.) But that this alteration acts more upon small spurious diameters, and less upon large ones.

Aug. 7, 1783. I tried some excessively small stars near γ

Aquilæ. When γ was perfectly distinct and round, the extremely small stars were dusky and ill defined; the *excessively* small ones were still less defined. As there are stars of all sizes in this neighbourhood, I saw some so very minute, that they only had the appearance of a small dusky spot, approaching to mere nebulosity. By very long attention I perceived many small dusky nebulous spots, which had it not been for this attention might have been in the field of view without the least suspicion.

(10.) From this we find that stars, when they are extremely small, lose their spurious diameters, and become nebulous.

July 7, 1780. I saw the spurious diameter of Arcturus gradually diminished by a haziness of the atmosphere till it vanished intirely.

A more circumstantial account of this observation has already been given; and some other causes that affect the spurious diameter of the stars, have been pointed out in the same paper, such as tremulous air, wind, and hoar-frost.*

January 31, 1788. The star in the back of Columba makes a spectrum, about 3′ or 6′ long, and about 2″ broad, finely coloured by the prismatic power of the atmosphere at this altitude.

July 28, 1783. Fomalhaut gives a beautiful prismatic spectrum, on account of its low situation.

July 17, 1781. With a new lens, power between 5 and 6 hundred, I saw ζ Aquarii, and found the vacancy between the two stars exactly 2 diameters. With my old one, power only 460, it was full 2 diameters. As it should have been larger with the high power than with the low one, it shows that the best eye-lens will give the least spurious diameter.

* See Phil. Trans. for 1803, page 224.

Oct. 12, 1782. I tried a new plain speculum, made by a very good workman, and found that when I viewed α Geminorum with 460, the vacancy between the two stars was barely 1½ diameter, but the same telescope and power with my own small speculum, made the distance 2 diameters, so that the figure of this mirror affects the spurious diameters of the stars.

(11.) Hence we may conclude that many causes will have an influence on the apparent diameter of the spurious disks of the stars; but they are so far within the reach of our knowledge, that with a proper regard to them, the conclusion we have drawn in Rem. (2.) "that under the same circumstances "their dimensions are permanent," will still remain good.

## SPURIOUS DIAMETERS OF TERRESTRIAL OBJECTS, WITH SIMILAR REMARKS.

### *7th Experiment with Silver Globules.*

A number of silver globules were put on the post, before they had been tarnished; and the sun shone upon them. When I viewed them in the telescope, there was on each of them a lucid appearance resembling the spurious disk of a star. I could distinguish this bright spot from the real diameters of the globules perfectly well, and found it much less than they were.

REM. (1.) Hence we conclude that the terrestrial, spurious disks of globules are less than the real disks; whereas we have seen, in Remark (1.) of the celestial spurious disks, that these are larger than the real ones.

*8th Experiment.*

The luminous spots, or spurious disks of the globules were of unequal diameters. The globule No. 1 had the largest disk, and the smaller ones the least; and the gradation of the sizes followed the order of the numbers.

(2.) This agrees with the spurious disks of celestial objects: the stars of the first, second, and third magnitude having a larger spurious disk than those that are of inferior magnitudes.

*9th Experiment.*

I found that there was a considerable difference in the colour of the spurious disks; one of them was of a beautiful purple colour, another was inclined to orange, a large one was straw coloured, a small one pale-ash coloured, and most of them were bluish-white.

(3.) With respect to colours, therefore, the terrestrial also agree with the celestial spurious disks.

*10th Experiment.*

I made two globules of different diameters, and placed them very near each other, so that their spurious disks might resemble those of a double star; this succeeded perfectly well. I viewed them with different powers.

With 177, the vacancy between them is $\frac{1}{4}$ diameter of the large star.

With 232, it is $1\frac{1}{4}$ diameter.

With 303,8, it is $1\frac{1}{8}$ diameter.

With 432,3, it is $1\frac{1}{2}$.

(4.) This experiment proves that the spurious diameters

of the globules are also in this respect like the spurious disks of the stars; for they are proportionally lessened by increasing the magnifying power, and increased when the power is lowered.

(5.) When the estimations are compared with the powers, it will also be seen that the increase and decrease of the spurious disks of the globules is not inversely as the powers, but in a much less ratio.

*11th Experiment.*

Two other globules of different sizes were examined; and

With 706,8 they were pretty unequal.

With 522,7 they were considerably unequal.

With 303,8 they were very unequal.

(6.) This proves that the effect of magnifying power is unequally exerted on spurious diameters; and that, as with celestial objects, so with terrestrial, this power acts more on the small spurious disks than on the large ones.

*12th Experiment.*

I viewed a different artificial double star with 522,7, and keeping always the same power, changed the aperture of the telescope.

With the inside rays I found them considerably unequal, and $2\frac{1}{2}$ diameters of the largest asunder. The spurious disks are perfectly well defined, round, and of a planetary aspect.

With all the mirror open, they are also round and well defined.

With the outside rays, they are near 4 diameters of the largest asunder, and are also round and distinct, but surrounded with flashing rays and bright nodules in continual motion.

(7.) This shows that the spurious terrestrial disks, in this respect again resemble those of the stars; increasing when the aperture is lessened, and decreasing when it is enlarged.

*13th Experiment.*

With the same magnifying power 482,3, but a change of aperture, I viewed two equal globules, and two unequal ones.

With the inside rays the equal globules were 1 diameter asunder.

With all the mirror open, they were $1\frac{1}{2}$ diameter asunder.

And with the outside rays they were 2 diameters asunder.

The unequal globules, with the inside rays, were a little unequal, and 1 diameter of the large one asunder.

With the outside rays they were considerably unequal, and 2 diameters of the large one asunder.

(8.) By these experiments it is proved, that the increase and decrease of the diameters occasioned by different apertures is not proportional to the diameters of the spurious disks.

(9.) But that the change of the apertures acts more on the small, and less on the large ones.

*14th Experiment.*

No. 1 of a set of globules, has the largest spurious diameter. No. 3 is larger than No. 2; whereas No. 2 has the largest real diameter. It is inclined to a greenish colour. No. 3 is now reddish, and is larger than No. 1, which is at present less than No. 2. No. 1 grows bigger, and is now the largest.

The sun which had been shining, was obscured by some clouds, but the spurious diameters of the globules I was viewing

remained visible, and were almost as bright as when the sun shone upon them.

I saw one of the globules lose its spurious diameter while the sun continued to shine. After some time the spurious diameter came on again, and very gradually grew brighter, but not larger. The colour of one of the globules being of a beautiful purple, changed soon after to a brilliant white.

The sun being obscured by some clouds, a globule lost its spurious diameter, and acquired the shape of an half moon, of the size of the real disk or diameter of the globule. I saw the sun break out again, and the half moon was gradually transformed into a much smaller spurious disk.

(10.) The spurious disks of globules are lost for want of proper illumination, but do not change their magnitude on that account. The brightness of the atmosphere in a fine day is sufficient to produce them; though the illumination of the sun is generally the principal cause of them.

(11.) The diameters of spurious disks are liable to change from various causes; an alteration in the direction of the illumination will make the reflection come from a different part of the globule, which can hardly be expected to be equally polished in its surface, or of equal convexity every where, being very seldom perfectly spherical; but as upon the whole the figure of them is pretty regular, the apparent diameter of the spurious disks will generally return to its former size.

*15th Experiment, with Drops of Quicksilver.*

At a time of the year when bright sun-shine is not very frequent, I found that my silver globules would seldom give

me an opportunity for experiments on spurious disks; to obviate this inconvenience, I used small drops of quicksilver. They are more lucid, and will give a bright spot with very little sunshine. Many of these drops of all sizes were exposed upon a plate of glass, and some on slips of steel. The management of them is a little different from that of the globules. For in order to represent a double star these must be placed one almost behind the other, as otherwise they cannot be brought near enough without running together. The following general observation will include all the necessary particulars.

The bright spots on drops of quicksilver are very small compared to the size of the drops.

They are not proportional to the magnitude of the drops, though less on the small ones and greater on large ones.

In some of the large ones the bright spot is about $\frac{1}{30}$ or $\frac{1}{40}$ of the diameter of the drop.

The magnitude of the luminous spots is liable to changes, but is rather more permanent than with the silver globules.

There is a little difference in the colour of the luminous spots; they are generally of a brilliant white, but sometimes they incline to yellow, and the small ones to ash-colour.

With high magnifying powers they are very well defined, and, on account of their brightness, will bear these powers better than the silver globules.

If M and $m$, stand for the diameters of the large and small mirror of my telescope, then will an aperture $= \sqrt{\frac{M^2 - m^2}{2} + m^2}$ give half the light of the telescope. With this I examined two of the drops, and found the luminous spots upon them with

925,4 nearly equal, and 2½ diameters of the largest asunder.

706,3 nearly equal, and above 2 diameters of the largest asunder.

432,3 pretty unequal, and 2 diameters of the largest asunder.

177,0 considerably unequal, and 1¼ diameter of the largest asunder.

I examined also two other drops, with different apertures, without changing the power, which was 706,3.

With the inside rays they were very little unequal, and ¼ diameter of the largest asunder.

With the outside rays they were considerably unequal, and 1¼ diameter of the largest asunder.

From what has been said, it appears that all the remarks which have been made with regard to the spurious disks of the silver globules are confirmed by the luminous spots on the drops of quicksilver. There is a difference in the proportion which the spurious disks on quicksilver bear to the drops, and that on the silver globules to the size of the globules; the latter also give a greater variety of colours and magnitudes than those on quicksilver; these are circumstances of which it would be easy to assign the cause, but they can be of no consequence to the result we have drawn from the experiment.

*16th Experiment, with black and white Circles.*

I tried to measure some of the spurious disks by projecting them on a scale with a moveable index, but found their diameters were too small for accuracy by this method; for this reason I had recourse to artificial measuring-disks, and prepared a set of eleven white circles on a black ground, and eight black ones on a white ground. In order to guard against

deceptions, I fixed them up against a tablet 154 inches from the eye, where it was intended to project the spurious disks of the globules, and examined them at that distance with the naked eye. Comparing then the size of the black to the white, I judged No. 1 of the black to be a little larger than No. 6 of the white circles. By a measure taken afterwards, it appeared that the black one was ,40 and the white ,89. Without supposing that every estimation may be made at this distance with equal accuracy, to the hundredth part of an inch, it is sufficiently evident that no material deception can take place in estimating by either of the sets of circles on account of their colour.

*17th Experiment, with different Illumination.*

A similar experiment was made in the microscope, by which the globules were measured. Two of them were placed on the measuring stand, and with an illumination from below, they appeared black, and were projected on white paper. The diameter of each globule and the distance between them were then measured. After this, I caused the illumination to come from above, and the globules being now of a silvery white, were projected on a slate. In this situation, when I repeated the former measures, no difference could be perceived.

*18th Experiment. Measures of spurious Disks.*

The spurious disk of a globule was then projected on the tablet where the white circles were placed. While I was comparing it with No. 4, which is ,81 in diameter and estimated it to be a little less than the circle, the spurious disk grew brighter; but it remained still of the same size; so that a variation in the quantity of the illumination will make no difference.

Every thing being now arranged for the measurement, I viewed the spurious diameter, with a magnifying power of 522,7, and compared it to the circles which succeeded each other by small differences of magnitude.

With all the mirror, from the centre to 8,8 inches open, the diameter of the spurious disk was ,31 inches.

With 6,3 inches open, it was less than ,40 and larger than ,355.

With 5 inches open, it was ,40.

With 4 inches open, it was ,42.

With 3 inches open, it was ,465 nearly.

From these measures it might be supposed that by lessening the quantity of light, we bring on a certain indistinctness which gives more diameter to the spurious object; to prove that this is not the cause of the increase, I used the following apertures.

With an annular opening from 6,5 to 8,8 inches, the spurious disk was rather less than ,18.

With another from 5 to 8,8 it was exactly ,18.

With an opening from 4 to 6,5 it was ,22.

With another from 1,6 to 4 it was ,42.

(12.) Now since the outside rim from 6,5 to 8,8, which reflected less than half the light of the mirror, produced a spurious disk less than ,18 in diameter, and the whole light as we have seen gave a disk of ,31, it is evidently not the quantity of the light, but the part of the mirror from which it is reflected, that we are to look upon as the cause of the magnitude of the spurious disks of objects.

(13.) These measures therefore point out an improvement in my former method of putting any terrestrial disk we suspect to be spurious to the test. For the inside rays of a mirror, as

before, will increase the diameter of these disks, but the outside rays alone will have a greater effect in reducing it, than when the inside rays are left to join with them.

### 19*th Experiment. Trial of Estimations.*

I placed two silver globules at a small distance from each other upon the post, but without measuring either the globules or their distance. When I viewed them with 522,7 they appeared in the shape of two half moons in an horizontal situation. The unenlightened parts of them were also pretty distinctly visible. I estimated the vacancy between the cusps of the lunes to be $\frac{1}{4}$ diameter of the largest.

On measuring the diameters and distance under the microscope, it appeared that the largest was ,0312; a quarter of which is ,0078. The distance of the globules from each other measured ,0111. The difference in the estimation ,0033 is less than $\frac{1}{300}$ part of an inch.

The experiment was repeated with a change of the distance of the globules from each other. They were then estimated to be less than the diameter of the large one asunder, but full that of the small one. When they were measured it was found that their distance was ,02608, and the diameter of the small one was ,0247, which estimation is still more accurate than the former.

### 20*th Experiment. Use of the Criterion.*

It remained now to be ascertained whether these half moons were spurious or real; for although I could also imperfectly perceive the dark part of the disks of the globules, yet a doubt would arise whether the two halves were really of equal

magnitude; to resolve this question, I viewed them first with the inside rays of the mirror, then with the outside, and found that in both cases the distance of the lines remained without the least alteration. I viewed them also with the whole mirror open, but it occasioned no change.

*21st Experiment. Measures of the comparative Amount of the spurious Diameters, produced by the Inside and Outside Rays.*

I divided the aperture of the mirror into two parts, one from 0 to 4,4 and the other from 4,4 to 8,8 inches. When I measured the spurious diameter of a globule, the inside rays made it ,40; with all the mirror open it was ,81; and with the outside rays it was ,92.

(14.) From this we may conclude that the diameters given by the inside rays, by all the mirror open, and by the outside rays, are in an arithmetical progression; and that the inside rays will nearly double, the diameter given by the outside. It remains however to be ascertained whether this will hold good with spurious disks of various magnitudes.

It will not be necessary to carry the divisions of the aperture farther; for as the application of these experiments is chiefly intended for astronomical purposes, we can hardly do with less than half the mirror open; and on the other hand with a very narrow rim of reflection from the outside of the mirror, distinctness would be apt to fail.

*22d Experiment. Trial of the Criterion on celestial Objects.*

I viewed $\alpha$ Lyræ with the outside rays, and found its spurious disk to be small; with all the mirror open it was larger, and with the inside rays it was largest.

As far as the imagination will enable us to compare objects we see in succession, the magnitudes appeared to be in an arithmetical progression.

*23d Experiment.*

I examined α Geminorum with 410,5, and with the outside rays the stars were considerably unequal, and $1\frac{1}{4}$ diameter of the largest asunder. With all the mirror open they were more unequal, and $1\frac{1}{2}$ diameter of the largest. With the inside rays they were very unequal, and $1\frac{7}{8}$ of the largest asunder.

These experiments show that, if it had not been known that the apparent disks of the stars were spurious, the application of the improved criterion of the apertures would have discovered them to be so; and that consequently the same improvement is perfectly applicable to celestial objects.

## OBSERVATIONS ON THE NATURE AND MAGNITUDE OF MR. HARDING'S LATELY DISCOVERED STAR.

It will be remembered that in a former Paper, where I investigated the nature of the two asteroids discovered by Signior Piazzi and Dr. Olbers, I suggested the probability that more of them would soon be found out; it may therefore be easily supposed that I was not much surprised when I was informed of Mr. Harding's valuable discovery.

On the day I received an account of it, which was the 24th of September, I directed my telescope to the calculated place of the new object, and noted all the small stars within a limited compass about it. They were then examined with a distinct high magnifying power; and since no difference in their appearance was perceivable, it became necessary to attend to the changes that

might happen in the situation of any one of them. They were delineated as in Fig. 1, (Plate I.) which is a mere eye-draught, to serve as an elucidation to a description given with it in the journal; and the star marked *k*, as will be seen hereafter, was the new object.

Sept. 25. The moon was too bright to see minute objects well, and my description the night before, for the same reason, had not been sufficiently particular; nor did I expect, from the account received, that the star had retrograded so far in its orbit.

Sept. 26. The weather being very hazy, no regular observations could be made; but as I noticed very particularly a star not seen before, it was marked *l* in Fig. 2, and proved afterwards to have been the lately discovered one, though still unknown this evening, for want of fixed instruments.

Sept. 27. I was favoured with Dr. MASKELYNE's account of the place of the star, taken at the Royal Observatory, by which communication I soon found out the object I was looking for.

Sept. 29. Being the first clear night, I began a regular series of observations; and as the power of determining small angles, and distinctness in showing minute disks, whether spurious or real, of the instrument I used on this occasion, has been sufficiently investigated by the foregoing experiments, there could be no difficulty in the observation, with resources that were then so well understood, and have now been so fully ascertained.

"Mr. HARDING's new celestial body precedes the very "small star in Fig. 3, between 29 and 33 Piscium, and is a "little larger than that star; it is marked A. *f g h* are taken "from Fig. 1. I suppose *g* to be of about the 9th magnitude,

"so that the new star may be called a small one of the "8th."

With the 10-feet reflector, power 496.3, I viewed it attentively, and comparing it with *g* and *h*, Fig. 3, could find no difference in the appearance but what might be owing to its being a larger star.

By way of putting this to a trial, I changed the power to 879.4, but could not find that it magnified the new one more than it did the stars *g* and *h*.

"I cannot perceive any disk; its apparent magnitude with "this power is greater than that of the star *g*, and also a very "little greater than that of *h*; but in the finder, and the night-"glass *g* is considerably smaller than the new star, and *h* is "also a very little smaller."

I compared it now with a star which in the finder appeared to be a very little larger, and in the telescope with 879.4 the apparent magnitude of this star was also larger than that of the new one.

"As far as I can judge without seeing the asteroids of Mr. "PIAZZI and Dr. OLBERS at the same time with Mr. HARDING's, "the last must be at least as small as the smallest of the "former, which is that of Dr. OLBERS."

"The star *k*, Fig. 1, observed Sept. 24, is wanting, and was "therefore the object I was in search of, which by computation "must have been that day in the place where I saw it."

"The new star being now in the meridian with all those to "which I am comparing it, and the air at this altitude being "very clear, I still find appearances as before described: the "new object cannot be distinguished from the stars by mag-"nifying power, so that this celestial body is a true ASTEROID."

Mr. BODE's stars 19, 25 and 27 Ceti are marked 7m, and by comparing the asteroid, which I find is to be called Juno, with these stars, it has the appearance of a small one of the 8th magnitude.

With regard to the diameter of Juno, which name it will at present be convenient to use, leaving it still to astronomers to adopt any other they may fix upon, it is evident that, had it been half a second, I must have instantly perceived a visible disk. Such a diameter, when I saw it magnified 879,4 times, would have appeared to me under an angle of 7′ 19″,7, one half of which, it will be allowed, from the experiments that have been detailed, could not have escaped my notice.

Oct. 1. Between flying clouds, I saw the asteroid, which in its true starry form has left the place where I saw it Sept. 29. It has taken the path in which by calculation I expected it would move. This ascertains that no mistake in the star was made when I observed it last.

Oct. 2, 7h. Mr. HARDING's asteroid is again removed, but is too low for high powers.

8h 30′. I viewed it now with 220,3 288,4 410,5 496,8 and 879,4. No other disk was visible than that spurious one which such small stars have, and which is not proportionally magnified by power.

With 288,4, the asteroid had a larger spurious disk than a star which was a little less bright, and a smaller spurious disk than another star that was a little more bright.

Oct. 5, with 410,5. The situation of the asteroid is now as in Fig. 4. I compared its disk, which is probably the spurious appearance of stars of that magnitude, with a larger, an equal, and a smaller star. It is less than the spurious disk of the

larger, equal to that of the equal, and larger than that of the smaller star. The gradual difference between the three stars is exceedingly small.

"With 496,9, and the air uncommonly pure and calm, I see "so well that I am certain the disk, if it be not a spurious one, "is less than one of the smallest globules I saw this morning "in the tree."

The diameter of this globule was ,08. It subtended an angle of 0",429, and was of sealing-wax; had it been a silver one, it would have been still more visible.

With 879,4. All comparative magnitudes of the asteroid and stars, remain as with 496,9.

I see the minute double star $\gamma$ Ophiuchi * in high perfection, which proves that the air is clear, and the telescope in good order.

The asteroid being now in the meridian, and the air very pure, I think the comparative diameter is a little larger than that of an equal star, and its light also differs from star-light. Its apparent magnitude, however, can hardly be equal to that of the smallest globule I saw this morning. This globule measured ,01338, and at the distance of 9620,4 inches subtended an angle of 0",214.

When I viewed the asteroid with 879,4 I found more haziness than an equal star would have given: but this I ascribe to want of light. What I call an equal star, is one that in an achromatic finder appears of equal light.

Oct. 7. Mr. HARDING's asteroid has continued its retrograde motion. The weather is not clear enough to allow the use of high powers.

* See Cat. of double Stars, I. 87.

Oct. 8. If the appearance resembling the spurious disks of small stars, which I see with 410,5 in Mr. HARDING's asteroid, should be a real diameter, its quantity then by estimation may amount to about 0″,8. This judgment is founded on the facility with which I can see two globules often viewed for this purpose.

The angle of the first is 0″,429, and of the other 0″,214; and the asteroid might be larger than the latter, but certainly was not equal to the former.

With 496,3, there is an ill defined hazy appearance, but nothing that may be called a disk visible. When there is a glimpse of more condensed light to be seen in the centre, it is so small that it must be less than two-tenths of a second.

To decide whether this apparent condensed light was a real or spurious disk, I applied different limitations to the aperture of the telescope, but found that the light of the new star was too feeble to permit the use of them. From this I concluded that an increase of light might now be of great use, and viewed the asteroid with a fine 10-feet mirror of 24 inches diameter, but found that nothing was gained by the change. The temperature indeed of these large mirrors is very seldom the same as that of the air in which they are to act, and till a perfect uniformity takes place, no high powers can be used.

The asteroid in the meridian, and the night beautiful. After many repeated comparisons of equal stars with the asteroid, I think it shows more of a disk than they do, but it is so small that it cannot amount to so much as 3-tenths of a second, or at least to no more.

It is accompanied with rather more nebulosity than stars of the same size.

The night is so clear, that I cannot suppose vision at this altitude to be less perfect on the stars, than it is on day objects at the distance of 800 feet in a direction almost horizontal.

Oct. 11. By comparing the asteroid alternately and often with equal stars, its disk, if it be a real one, cannot exceed 2, or at most 3-tenths of a second. This estimation is founded on the comparative readiness with which every fine day I have seen globules subtending such angles in the same telescope, and with the same magnifying power.

"The asteroid is in the meridian, and in high perfection. I "perceive a well defined disk that may amount to 2 or 3-tenths "of a second; but an equal star shows exactly the same ap- "pearance, and has a disk as well defined and as large as that "of the asteroid."

## RESULT AND APPLICATION OF THE EXPERIMENTS AND OBSERVATIONS.

We may now proceed to draw a few very useful conclusions from the experiments that have been given, and apply them to the observations of the star discovered by Mr. HARDING; and also to the similar stars of Mr. PIAZZI and Dr. OLBERS. These kind of corollaries may be expressed as follows.

(1.) A 10-feet reflector will show the spurious or real disks, of celestial and terrestrial objects, when their diameter is $\frac{1}{4}$ of a second of a degree; and when every circumstance is favourable, such a diameter may be perceived so distinctly, that it can be divided by estimation into two or three parts.

(2.) A disk of $\frac{1}{4}$ of a second in diameter, whether spurious or real, in order to be seen as a round, well defined body,

requires a distinct magnifying power of 5 or 6 hundred, and must be sufficiently bright to bear that power.

(3.) A real disk of half a second in diameter will become so much larger by the application of a magnifying power of 5 or 6 hundred, that it will be easily distinguished from an equal spurious one, the latter not being affected by power in the same proportion as the former.

(4.) The different effects of the inside and outside rays of a mirror, with regard to the appearance of a disk, are a criterion that will show whether it is real or spurious, provided its diameter is more than $\frac{1}{4}$ of a second.

(5.) When disks, either spurious or real, are less than $\frac{1}{4}$ of a second in diameter, they cannot be distinguished from each other; because the magnifying power will not be sufficient to make them appear round and well defined.

(6.) The same kind of experiments are applicable to telescopes of different sorts and sizes, but will give a different result for the quantity which has been stated at $\frac{1}{4}$ of a second of a degree. This will be more when the instrument is less perfect, and less when it is more so. It will also differ even with the same instrument, according to the clearness of the air, the condition, and adjustment of the mirrors, and the practical habits of the observer.

---

With regard to Mr. HARDING's new starry celestial body, we have shown, by observation, that it resembles, in every respect, the two other lately discovered ones of Mr. PIAZZI and Dr. OLBERS; so that Ceres, Pallas, and Juno, are certainly three individuals of the same species.

That they are beyond comparison smaller than any of the seven planets cannot be questioned, when a telescope that will show a diameter of $\frac{1}{4}$ of a second of a degree, leaves it undecided whether the disk we perceive is a real or a spurious one.

A distinct magnifying power, of more than 5 or 6 hundred, has been applied to Ceres, Pallas, and Juno, but has either left us in the dark, or at least has not fully removed every doubt upon this subject.

The criterion of the apertures of the mirror, on account of the smallness of these objects, has been as little successful; and every method we have tried has ended in proving their resemblance to small stars.

It will appear, that when I used the name asteroid to denote the condition of Ceres and Pallas, the definition I then gave of this term* will equally express the nature of Juno, which, by its similar situation between Mars and Jupiter, as well as by the smallness of its disk, added to the considerable inclination and excentricity of its orbit, departs from the general condition of planets. The propriety therefore of using the same appellation for the lately discovered celestial body cannot be doubted.

Had Juno presented us with a link of a chain, uniting it to those great bodies, whose rank in the solar system I have also defined,† by some approximation of a motion in the zodiac, or by a magnitude not very different from a planetary one, it might have been an inducement for us to suspend our judg-

* See Phil. Trans. for 1802, p. 229, line 10.

† Ibid. page 224, line 3 of the same Paper.

ment with respect to a classification; but the specific difference between planets and asteroids appears now by the addition of a third individual of the latter species to be more fully established, and that circumstance, in my opinion, has added more to the ornament of our system than the discovery of another planet could have done.

Slough, near Windsor,
Dec. 1, 1804.

Fig. 1.

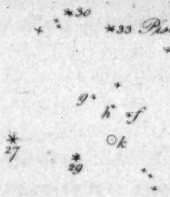



Fig. 2.

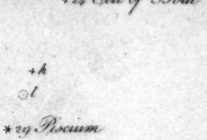
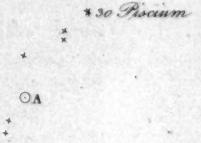



Fig. 3.

Fig. 4.




Basire sc.

## III. *An Essay on the Cohesion of Fluids.* By Thomas Young, *M.D. For. Sec. R.S.*

Read December 20, 1804.

### *I. General Principles.*

It has already been asserted, by Mr. Monge and others, that the phenomena of capillary tubes are referable to the cohesive attraction of the superficial particles only of the fluids employed, and that the surfaces must consequently be formed into curves of the nature of lintearie, which are supposed to be the results of a uniform tension of a surface, resisting the pressure of a fluid, either uniform, or varying according to a given law. Segner, who appears to have been the first that maintained a similar opinion, has shown in what manner the principle may be deduced from the doctrine of attraction, but his demonstration is complicated, and not perfectly satisfactory; and in applying the law to the forms of drops, he has neglected to consider the very material effects of the double curvature, which is evidently the cause of the want of a perfect coincidence of some of his experiments with his theory. Since the time of Segner, little has been done in investigating accurately and in detail the various consequences of the principle.

It will perhaps be most agreeable to the experimental philosopher, although less consistent with the strict course of logical argument, to proceed in the first place to the comparison

of this theory with the phenomena, and to inquire afterwards for its foundation in the ultimate properties of matter. But it is necessary to premise one observation, which appears to be new, and which is equally consistent with theory and with experiment; that is, that for each combination of a solid and a fluid, there is an appropriate angle of contact between the surfaces of the fluid, exposed to the air, and to the solid. This angle, for glass and water, and in all cases where a solid is perfectly wetted by a fluid, is evanescent: for glass and mercury, it is about 140°, in common temperatures, and when the mercury is moderately clean.

## II. *Form of the Surface of a Fluid.*

It is well known, and it results immediately from the composition of forces, that where a line is equably distended, the force that it exerts, in a direction perpendicular to its own, is directly as its curvature; and the same is true of a surface of simple curvature; but where the curvature is double, each curvature has its appropriate effect, and the joint force must be as the sum of the curvatures in any two perpendicular directions. For this sum is equal, whatever pair of perpendicular directions may be employed, as is easily shown by calculating the versed sines of two equal arcs taken at right angles in the surface. Now when the surface of a fluid is convex externally, its tension is produced by the pressure of the particles of the fluid within it, arising from their own weight, or from that of the surrounding fluid; but when the surface is concave, the tension is employed in counteracting the pressure of the atmosphere, or, where the atmosphere is excluded, the equivalent pressure arising from the weight of the particles suspended

from it by means of their cohesion, in the same manner as, when water is supported by the atmospheric pressure in an inverted vessel, the outside of the vessel sustains a hydrostatic pressure proportionate to the height; and this pressure must remain unaltered, when the water, having been sufficiently boiled, is made to retain its situation for a certain time by its cohesion only, in an exhausted receiver. When, therefore, the surface of the fluid is terminated by two right lines, and has only a simple curvature, the curvature must be every where as the ordinate; and where it has a double curvature, the sum of the curvatures in the different directions must be as the ordinate. In the first case the curve may be constructed by approximation, if we divide the height at which it is either horizontal or vertical into a number of small portions, and taking the radius of each portion proportional to the reciprocal of the height of its middle point above or below the general surface of the fluid, go on to add portions of circles joining each other, until they have described as much of the curve as is required. In the second case, it is only necessary to consider the curve derived from a circular basis, which is a solid of revolution; and the centre of that circle of curvature, which is perpendicular to the section formed by a plane passing through the axis, is in the axis itself, consequently in the point where the normal of the curve intersects the axis: we must therefore here make the sum of this curvature, and that of the generating curve, always proportional to the ordinate. This may be done mechanically, by beginning at the vertex, where the two curvatures are equal, then, for each succeeding portion, finding the radius of curvature by deducting the proper reciprocal of the normal, at the beginning of the portion, from the ordinate,

and taking the reciprocal of the remainder. In this case the analysis leads to fluxional equations of the second order, which appear to afford no solution by means hitherto discovered; but the cases of simple curvature may be more easily subjected to calculation.

III. *Analysis of the simplest Forms.*

Supposing the curve to be described with an equable angular velocity, its fluxion, being directly as the radius of curvature, will be inversely as the ordinate, and the rectangle contained by the ordinate and the fluxion of the curve will be a constant quantity; but this rectangle is to the fluxion of the area, as the radius to the cosine of the angle formed by the curve with the horizon; and the fluxion of the area varying as the cosine, *the area itself will vary as the sine of this angle, and will be equal to the rectangle contained by the initial ordinate, and the sine corresponding to each point of the curve in the initial circle of curvature.* Hence it follows, that *the whole area included by the ordinates where the curve is vertical and where it is horizontal, is equal to the rectangle contained by the ordinate and the radius of curvature;* and, secondly, that the area on the convex side of the curve, between the vertical tangent and the least ordinate, is equal to the whole area on the concave side of the curve between the same tangent and the greatest ordinate.

In order to find the ordinate corresponding to a given angular direction, we must consider that the fluxion of the ordinate at the vertical part, is equal to the fluxion of the circle of curvature there, that, in other places, it varies as the radius of curvature and the sine of the angle formed with the horizon

conjointly, or as the ordinate inversely, and directly as the sine of elevation; therefore the fluxion of the ordinate multiplied by the ordinate is equal to the fluxion of any circle of curvature multiplied by its corresponding height, and by the sine, and divided by the radius: but the fluxion of the circle multiplied by the sine and divided by the radius, is equal to the fluxion of the versed sine; therefore the ordinate multiplied by its fluxion is equal to the initial height multiplied by the fluxion of the versed sine in the corresponding circle of curvature; and *the square of the ordinate is equal to the rectangle contained by the initial height and twice the versed sine, increased by a constant quantity.* Now at the highest point of the curve, the versed sine becomes equal to the diameter, and the square of the initial height to the rectangle contained by the initial height and twice the diameter, with the constant quantity: the constant quantity is therefore equal to the rectangle contained by the initial height and its difference from twice the diameter: *this constant quantity is the square of the least ordinate,* and *the ordinate is every where a mean proportional between the greatest height and the same height diminished by twice the versed sine of the angular depression in the corresponding circle of curvature.* Again, *at the vertical point, the square of the ordinate is equal to the square of the greatest height diminished by the rectangle contained by this height and the diameter of the corresponding circle of curvature,* a rectangle which is constant for every fluid, and which may be called *the appropriate rectangle: deducting this rectangle from the square of the ordinate at the vertical point, we have the least ordinate;* which consequently vanishes *when the square of the ordinate at the vertical point is equal to the appropriate rectangle; the horizontal surface becoming in this case an asymptote to the curve, and the*

*square of the greatest ordinate being equal to twice the appropriate rectangle, and the greatest ordinate to twice the diameter of the corresponding circle of curvature*; so that, *if we suppose a circle to be described, having this ordinate for a diameter, the chord of the angular elevation in this circle will be always equal to the ordinate at each point*, and *the ordinate will vary as the sine of half the angle of elevation, whenever the curve has an asymptote.* Mr. Fuss has demonstrated, in the third volume of the *Acta Petropolitana*, some properties of the arch of equilibrium under the pressure of a fluid, which is the same as one species of the curves here considered. The series given by Euler in the second part of the same volume, for the elastic curve, may also be applied to these curves.

IV. *Application to the Elevation of particular Fluids.*

The simplest phenomena, which afford us data for determining the fundamental properties of the superficial cohesion of fluids, are their elevation and depression between plates and in capillary tubes, and their adhesion to the surfaces of solids which are raised in a horizontal situation to a certain height above the general surface of the fluids. When the distance of a pair of plates, or the diameter of a tube, is very minute, the curvature may be considered as uniform, and the appropriate rectangle may readily be deduced from the elevation, recollecting that the curvature in a capillary tube is double, and the height therefore twice as great as between two plates. In the case of the elevation of a fluid in contact with a horizontal surface, the ordinate may be determined from the weight required to produce a separation; and the appropriate rectangle may be found in this manner also, the angle of contact being

properly considered, in this as well as in the former case. It will appear that these experiments by no means exhibit an immediate measure of the mutual attraction of the solid and fluid, as some authors have supposed.

Sir Isaac Newton asserts, in his Queries, that water ascends between two plates of glass at the distance of one hundredth of an inch, to the height of about one inch; the product of the distance and the height being about .01; but this appears to be much too little. In the best experiment of Musschenbroek, with a tube, half of the product was .0196; in several of Weitbrecht, apparently very accurate, .0214. In Monge's experiments on plates, the product was 2.6 or 2.7 lines, about .0210. Mr. Atwood says that for tubes, the product is .0580, half of which is .0265. Until more accurate experiments shall have been made, we may be contented to assume .02 for the rectangle appropriate to water, and .04 for the product of the height in a tube by its bore. Hence, when the curve becomes infinite, is greatest ordinate is .2, and the height of the vertical portion, or the height of ascent against a single vertical plane .14, or nearly one-seventh of an inch.

Now when a horizontal surface is raised from a vessel of water, the surface of the water is formed into a lintearia to which the solid is a tangent at its highest point, and if the solid be still further raised, the water will separate: the surface of the water, being horizontal at the point of contact, cannot add to the weight tending to depress the solid, which is therefore simply the hydrostatic pressure of a column of water equal in height to the elevation, in this case one-fifth of an inch, and standing on the given surface. The weight of such a column will be $50\frac{1}{2}$ grains for each square inch; and

in TAYLOR's well known experiment the weight required was 50 grains. But when the solid employed is small, the curvature of the horizontal section of the water, which is convex externally, will tend to counteract the vertical curvature, and to diminish the height of separation: thus if a disc of an inch in diameter were employed, the curvature in this direction would perhaps be equivalent to the pressure of about one-hundredth of an inch, and might reduce the height from .4 to about .19, and the weight in the same proportion. There is however as great a diversity in the results of different experiments on the force required to elevate a solid from the surface of a fluid, as in those of the experiments in capillary tubes: and indeed the sources of error appear to be here more numerous. Mr. ACHARD found that a disc of glass, $1\frac{1}{2}$ inch French in diameter, required, at 69° of FAHRENHEIT, a weight of 91 French grains to raise it from the surface of water; this is only 87 English grains for each square inch; at $44\frac{1}{2}$° the force was $\frac{1}{12}$ greater, or $89\frac{1}{2}$ grains; the difference being $\frac{1}{313}$ for each degree of FAHRENHEIT. It might be inferred from these experiments, that the height of ascent in a tube of a given bore, which varies in the duplicate ratio of the height of adhesion, is diminished about $\frac{1}{160}$ for every degree of FAHRENHEIT that the temperature is raised above 50°; there was however probably some considerable source of error in ACHARD's experiments, for I find that this diminution does not exceed $\frac{1}{1000}$. The experiments of Mr. DUTOUR make the quantity of water raised equal to 44.1 grains for each square inch. Mr. ACHARD found the force of adhesion of sulfuric acid to glass, at 69° of FAHRENHEIT, 1.26, that of water being 1; hence the height was as .69 to 1, and its square as .47 to 1, which is the

corresponding proportion for the ascent of the acid in a capillary tube, and which does not very materially differ from the proportion of .395 to 1, assigned by BARRUEL for this ascent. MUSSCHENBROEK found it .8 to 1, but his acid was probably weak. For alcohol the adhesion was as .593, the height as .715, and its square as .510: the observed proportion in a tube, according to an experiment of MUSSCHENBROEK, was about .550, according to CARRE' from .400 to .440. The experiments on sulfuric ether do not agree quite so well, but its quality is liable to very considerable variations. DUTOUR found the adhesion of alcohol .58, that of water being 1.

With respect to mercury, it has been shown by Professor CASBOIS of Metz, and by others, that its depression in tubes of glass depends on the imperfection of the contact, and that when it has been boiled in the tube often enough to expel all foreign particles, the surface may even become concave instead of convex, and the depression be converted into an elevation. But in barometers, constructed according to the usual methods, the angle of the mercury will be found to differ little from 140°; and in other experiments, when proper precautions are taken, the inclination will be nearly the same. The determination of this angle is necessary for finding the appropriate rectangle for the curvature of the surface of mercury, together with the observations of the quantity of depression in tubes of a given diameter. The table published by Mr. CAVENDISH from the experiments of his father, Lord CHARLES CAVENDISH, appears to be best suited for this purpose. I have constructed a diagram, according to the principles already laid down, for each case, and I find that the rectangle which agrees best with the phenomena is .01. The mean depression is always .015,

divided by the diameter of the tube: and in tubes less than half an inch in diameter, the curve is very nearly elliptic, and the central depression in the tube of a barometer may be found by deducting from the corresponding mean depression the square root of one-thousandth part of its diameter. There is reason to suspect a slight inaccuracy towards the middle of Lord CHARLES CAVENDISH's Table, from a comparison with the calculated mean depression, as well as from the results of the mechanical construction. The ellipsis approaching nearest to the curve may be determined by the solution of a biquadratic equation.

| Diameter in inches. | Grains in an inch. C. | Mean depression by calculation. Y. | Central depression by observation. C. | Central depression by formula. Y. | Central depression by diagram. Y. | Marginal depression by diagram. Y. |
|---|---|---|---|---|---|---|
| .6 | 972 | .025 | .005 | (.001) | .005 | .066 |
| .5 | 675 | .030 | .007 | .008 | .007 | .067 |
| .4 | 432 | .037 | .015 | .017 | .012 | .069 |
| .35 | 331 | .043 | .025 | .024 | .017 | .072 |
| .30 | 243 | .050 | .036 | .033 | .027 | .079 |
| .25 | 169 | .060 | .050 | .044 | .038 | .086 |
| .20 | 108 | .075 | .067 | .061 | .056 | .096 |
| .15 | 61 | .100 | .092 | .088 | .085 | .116 |
| .10 | 27 | .150 | .140 | .140 | .140 | .161 |

The square root of the rectangle .01, or .1, is the ordinate where the curve would become vertical if it were continued; but in order to find the height at which it adheres to a vertical surface, we must diminish this ordinate in the proportion of the sine of 25° to the sine of 45°, and it will become .06, for the actual depression in this case. The elevation of the mercury that adheres to the lower horizontal surface of a piece of glass, and

the thickness at which a quantity of mercury will stand when spread out on glass, supposing the angle of contact still 140°, are found, by taking the proportion of the sines of 20° and of 70° to the sine of 48°, and are therefore .0484 and .1330 respectively. If, instead of glass, we employed any surface capable of being wetted by mercury, the height of elevation would be .141, and this is the limit of the thickness of a wide surface of mercury supported by a substance wholly incapable of attracting it. Now the hydrostatic pressure of a column of mercury .0484 in thickness on a disc of one inch diameter would be 191 grains; to this the surrounding elevation of the fluid will add about 11 grains for each inch of the circumference, with some deduction for the effect of the contrary curvature of the horizontal section, tending to diminish the height; and the apparent cohesion thus exhibited will be about 160 grains, which is a little more than four times as great as the apparent cohesion of glass and water. With a disc 11 lines in diameter Mr. Dutour found it 194 French grains, which is equivalent to 152 English grains, instead of 160, for an inch, a result which is sufficient to confirm the principles of the calculation. The depth of a quantity of mercury standing on glass I have found by actual observation, to agree precisely with this calculation. Segner says that the depth was .1358, both on glass and on paper: the difference is very trifling, but this measure is somewhat too great for glass, and too small for paper, since it appears from Dutour's experiments, that the attraction of paper to mercury is extremely weak.

If a disc of a substance capable of being wetted by mercury, an inch in diameter, were raised from its surface in a position perfectly horizontal, the apparent cohesion should be 381

grains, taking .141 as the height: and for a French circular inch, 438 grains, or 528 French grains. Now, in the experiments of MORVEAU, the cohesion of a circular inch of gold to the surface of mercury appeared to be 446 grains, of silver 429, of tin 418, of lead 397, of bismuth 372, of zinc 204, of copper 142, of metallic antimony 126, of iron 115, of cobalt 8: and this order is the same with that in which the metals are most easily amalgamated with mercury. It is probable that such an amalgamation actually took place in some of the experiments, and affected their results, for the process of amalgamation may often be observed to begin almost at the instant of contact of silver with mercury; and the want of perfect horizontality appears in a slight degree to have affected them all. A deviation of one-fiftieth of an inch would be sufficient to have produced the difference between 446 grains and 528; and it is not impossible that all the differences, as far down as bismuth, may have been accidental. But if we suppose the gold only to have been perfectly wetted by the mercury, and all the other numbers to be in due proportions, we may find the appropriate angle for each substance by deducting from 180°, twice the angle of which the sine is to the radius as the apparent cohesion of each to 446 grains; that is, for gold 1, for silver about .97, for tin .95, for lead .90, for bismuth .85, for zinc .46, for copper .32, for antimony .29, for iron .26, and for cobalt .02, neglecting the surrounding elevation, which has less effect in proportion as the surface employed is larger. GELLERT found the depression of melted lead in a tube of glass multiplied by the bore equal to about .0054.

It would perhaps be possible to pursue these principles so

far as to determine in many cases the circumstances under which a drop of any fluid would detach itself from a given surface. But it is sufficient to infer, from the law of the superficial cohesion of fluids, that the linear dimensions of similar drops depending from a horizontal surface must vary precisely in the same ratio as the heights of ascent of the respective fluids against a vertical surface, or as the square root of the heights of ascent in a given tube: hence the magnitudes of similar drops of different fluids must vary as the cubes of the square roots of the heights of ascent in a tube. I have measured the heights of ascent of water and of diluted spirit of wine in the same tube, and I found them nearly as 100 to 64: a drop of water falling from a large sphere of glass weighed 1.8 grains, a drop of the spirit of wine about .85, instead of .82, which is nearly the weight that would be inferred from the consideration of the heights of ascent, combined with that of the specific gravities. We may form a conjecture respecting the probable magnitude of a drop by inquiring what must be the circumference of the fluid, that would support by its cohesion the weight of a hemisphere depending from it: this must be the same as that of a tube, in which the fluid would rise to the height of one-third of its diameter; and the square of the diameter must be three times as great as the appropriate product; or, for water .12; whence the diameter would be .35, or a little more than one-third of an inch, and the weight of the hemisphere would be 2.8 grains. If more water were added internally, the cohesion would be overcome, and the drop would no longer be suspended, but it is not easy to calculate what precise quantity of water would be separated with it. The form of a bubble of air rising in water is determined

by the cohesion of the internal surface of the water exactly in the same manner as the form of a drop of water in the air. The delay of a bubble of air at the bottom of a vessel appears to be occasioned by a deficiency of the pressure of the water between the air and the vessel; it is nearly analogous to the experiment of making a piece of wood remain immersed in water, when perfectly in contact with the bottom of the vessel containing it. This experiment succeeds however far more readily with mercury, since the capillary cohesion of the mercury prevents its insinuating itself under the wood.

### V. *Of apparent Attractions and Repulsions.*

The apparent attraction of two floating bodies, round both of which the fluid is raised by cohesive attraction, is produced by the excess of the atmospheric pressure on the remote sides of the solids above its pressure on their neighbouring sides: or, if the experiments are performed in a vacuum, by the equivalent hydrostatic pressure or suction derived from the weight and immediate cohesion of the intervening fluid. This force varies ultimately in the inverse ratio of the square of the distance; for, if two plates approach each other, the height of the fluid that rises between them is increased in the simple inverse ratio of the distance; and the mean action, or negative pressure, of the fluid on each particle of the surface is also increased in the same ratio. When the floating bodies are both surrounded by a depression, the same law prevails, and its demonstration is still more simple and obvious. The repulsion of a wet and a dry body does not appear to follow the same proportion: for it by no means approaches to infinity upon the supposition of perfect contact; its maximum is measured

by half the sum of the elevation and depression on the remote sides of the substances, and as the distance increases, this maximum is only diminished by a quantity, which is initially as the square of the distance. The figures of the solids concerned modify also sometimes the law of attraction, so that, for bodies surrounded by a depression, there is sometimes a maximum, beyond which the force again diminishes: and it is hence that a light body floating on mercury, in a vessel little larger than itself, is held in a stable equilibrium without touching the sides. The reason of this will become apparent, when we examine the direction of the surface necessarily assumed by the mercury in order to preserve the appropriate angle of contact, the tension acting with less force when the surface attaches itself to the angular termination of the float in a direction less horizontal.

The apparent attraction produced between solids by the interposition of a fluid does not depend on their being partially immersed in it; on the contrary, its effects are still more powerfully exhibited in other situations; and, when the cohesion between two solids is increased and extended by the intervention of a drop of water or of oil, the superficial cohesion of these fluids is fully sufficient to explain the additional effect. When wholly immersed in water, the cohesion between two pieces of glass is little or not at all greater than when dry: but if a small portion only of a fluid be interposed, the curved surface, that it exposes to the air, will evidently be capable of resisting as great a force as it would support from the pressure of the column of fluid that it is capable of sustaining in a vertical situation; and in order to apply this force, we must employ in the separation of the plates, as great a force as is equivalent

to the pressure of a column appropriate to their distance MORVEAU found that two discs of glass, 3 inches French in diameter, at the distance of one-tenth of a line, appeared to cohere with a force of 4719 grains, which is equivalent to the pressure of a column 23 lines in height: hence the product of the height and the distance of the plates is 2.3 lines, instead of 2.65, which was the result of MONGE's experiments on the actual ascent of water. The difference is much smaller than the difference of the various experiments on the ascent of fluids; and it may easily have arisen from a want of perfect parallelism in the plates; for there is no force tending to preserve this parallelism. The error, in the extreme case of the plates coming into contact at one point, may reduce the apparent cohesion to one half.

The same theory is sufficient to explain the law of the force by which a drop is attracted towards the junction of two plates inclined to each other, and which is found to vary in the inverse ratio of the square of the distance; whence it was inferred by NEWTON that the primitive force of cohesion varies in the simple inverse ratio of the distance, while other experiments lead us to suppose that cohesive forces in general vary in the direct ratio of the distance. But the difficulty is removed by considering the state of the marginal surface of the drop. If the plates were parallel, the capillary action would be equal on both sides of the drop: but when they are inclined, the curvature of the surface at the thinnest part requires a force proportionate to the appropriate height to counteract it; and this force is greater than that which acts on the opposite side. But if the two plates are inclined to the horizon, the deficiency may be made up by the hydrostatic weight of the drop itself;

and the same inclination will serve for a larger or a smaller drop at the same place. Now when the drop approaches to the line of contact, the difference of the appropriate heights for a small drop of a given diameter will increase as the square of the distance decreases; for the fluxion of the reciprocal of any quantity varies inversely as the square of that quantity: and, in order to preserve the equilibrium, the sine of the angle of elevation of the two plates must be nearly in the inverse ratio of the square of the distance of the drop from the line of contact, as it actually appears to have been in HAUKSBEE's experiments.

VI. *Physical Foundation of the Law of superficial Cohesion.*

We have now examined the principal phenomena which are reducible to the simple theory of the action of the superficial particles of a fluid. We are next to investigate the natural foundations upon which that theory appears ultimately to rest. We may suppose the particles of liquids, and probably those of solids also, to possess that power of repulsion, which has been demonstratively shown by NEWTON to exist in aeriform fluids, and which varies in the simple inverse ratio of the distance of the particles from each other. In airs and vapours this force appears to act uncontrolled; but in liquids, it is overcome by cohesive force, while the particles still retain a power of moving freely in all directions; and in solids the same cohesion is accompanied by a stronger or weaker resistance to all lateral motion, which is perfectly independent of the cohesive force, and which must be cautiously distinguished from it. It is simplest to suppose the force of cohesion nearly or perfectly constant in its magnitude, throughout the minute

distance to which it extends, and owing its apparent diversity to the contrary action of the repulsive force, which varies with the distance. Now in the internal parts of a liquid these forces hold each other in a perfect equilibrium, the particles being brought so near that the repulsion becomes precisely equal to the cohesive force that urges them together: but whenever there is a curved or angular surface, it may be found by collecting the actions of the different particles, that the cohesion must necessarily prevail over the repulsion, and must urge the superficial parts inwards with a force proportionate to the curvature, and thus produce the effect of a uniform tension of the surface. For, if we consider the effect of any two particles in a curved line on a third at an equal distance beyond them, we shall find that the result of their equal attractive forces bisects the angle formed by the lines of direction; but that the result of their repulsive forces, one of which is twice as great as the other, divides it in the ratio of one to two, forming with the former result an angle equal to one-sixth of the whole; so that the addition of a third force is necessary in order to retain these two results in equilibrium; and this force must be in a constant ratio to the evanescent angle which is the measure of the curvature, the distance of the particles being constant. The same reasoning may be applied to all the particles which are within the influence of the cohesive force: and the conclusions are equally true if the cohesion is not precisely constant, but varies less rapidly than the repulsion.

### VII. *Cohesive Attraction of Solids and Fluids.*

When the attraction of the particles of a fluid for a solid is less than their attraction for each other, there will be an

equilibrium of the superficial forces, if the surface of the fluid make with that of the solid a certain angle, the versed sine of which is to the diameter, as the mutual attraction of the fluid and solid particles is to the attraction of the particles of the fluid among each other. For, when the fluid is surrounded by a vacuum or by a gas, the cohesion of its superficial particles acts with full force in producing a pressure; but when it is any where in contact with a solid substance of the same attractive power with itself, the effects of this action must be as much destroyed as if it were an internal portion of the fluid. Thus, if we imagined a cube of water to have one of its halves congealed, without any other alteration of its properties, it is evident that its form and the equilibrium of the cohesive forces would remain undisturbed: the tendency of the new angular surface of the fluid water to contract would therefore be completely destroyed by the contact of a solid of equal attractive force. If the solid were of smaller attractive force, the tendency to contract would only be proportionate to the difference of the attractive forces or densities, the effect of as many of the attractive particles of the fluid being neutralised, as are equivalent to a solid of a like density or attractive power. For a similar reason, the tendency of a fluid to contract the sum of the surfaces of itself and a contiguous solid, will be simply as the density of the solid, or as the mutual attractive force of the solid and fluid. And it is indifferent whether we consider the pressure produced by these supposed superficial tensions, or the force acting in the direction of the surfaces to be compared. We may therefore inquire into the conditions of equilibrium of the three forces acting on the angular particles, one in the direction of the surface of the fluid only, a

second in that of the common surface of the solid and fluid, and the third in that of the exposed surface of the solid. Now, supposing the angle of the fluid to be obtuse, the whole superficial cohesion of the fluid being represented by the radius, the part which acts in the direction of the surface of the solid will be proportional to the cosine of the inclination; and this force, added to the force of the solid, will be equal to the force of the common surface of the solid and fluid, or to the differences of their forces; consequently, the cosine added to twice the force of the solid, will be equal to the whole force of the fluid, or to the radius: hence the force of the solid is represented by half the difference between the cosine and the radius, or by half the versed sine; or, if the force of the fluid be represented by the diameter, the whole versed sine will indicate the force of the solid. And the same result follows when the angle of the fluid is acute. Hence we may infer, that if the solid have half the attractive force of the fluid, the surfaces will be perpendicular; and this seems in itself reasonable, since two rectangular edges of the solid are equally near to the angular particles with one of the fluid, and we may expect a fluid to rise and adhere to the surface of every solid more than half as attractive as itself; a conclusion which CLAIRAUT has already inferred, in a different manner, from principles which he has but cursorily investigated, in his treatise on the figure of the earth.

The versed sine varies as the square of the sine of half the angle: the force must therefore be as the square of the height to which the fluid may be elevated in contact with a horizontal surface, or nearly as the square of the number of grains expressing the apparent cohesion. Thus, according to the experiments of MORVEAU, on the suppositions already premised,

we may infer that the mutual attraction of the particles of mercury being unity, that of mercury for gold will be .1 or more, that of silver about .94, of tin .90, of lead .81, of bismuth .72, of zinc .21, of copper .10, of antimony .08, of iron .07, and of cobalt .0004. The attraction of glass for mercury will be about one-sixth of the mutual attraction of the particles of mercury: but when the contact is perfect, it appears to be considerably greater.

Although the whole of this reasoning on the attraction of solids is to be considered rather as an approximation than as a strict demonstration, yet we are amply justified in concluding, that all the phenomena of capillary action may be accurately explained and mathematically demonstrated from the general law of the equable tension of the surface of a fluid, together with the consideration of the angle of contact appropriate to every combination of a fluid with a solid. Some anomalies, noticed by Musschenbroek and others, respecting in particular the effects of tubes of considerable lengths, have not been considered: but there is great reason to suppose that either the want of uniformity in the bore, or some similar inaccuracy, has been the cause of these irregularities, which have by no means been sufficiently confirmed to afford an objection to any theory. The principle, which has been laid down respecting the contractile powers of the common surface of a solid and a fluid, is confirmed by an observation which I have made on the small drops of oil which form themselves on water. There is no doubt but that this cohesion is in some measure independent of the chemical affinities of the substances concerned: tallow when solid has a very evident attraction for the water

out of which it is raised; and the same attraction must operate upon an unctuous fluid to cause it to spread on water, the fluidity of the water allowing this powerful agent to exert itself with an unresisted velocity. An oil which has thus been spread is afterwards collected, by some irregularity of attraction, into thin drops, which the slightest agitation again dissipates: their surface forms a very regular curve, which terminates abruptly in a surface perfectly horizontal: now it follows from the laws of hydrostatics, that the lower surface of these drops must constitute a curve, of which the extreme inclination to the horizon is to the inclination of the upper surface as the specific gravity of the oil to the difference between its specific gravity and that of water: consequently since the contractile forces are held in equilibrium by a force which is perfectly horizontal, their magnitude must be in the ratio that has been already assigned; and it may be assumed as consonant both to theory and to observation, that the contractile force of the common surface of two substances, is proportional, other things being equal, to the difference of their densities. Hence, in order to explain the experiments of BOYLE on the effects of a combination of fluids in capillary tubes, or any other experiments of a similar nature, we have only to apply the law of an equable tension, of which the magnitude is determined by the difference of the attractive powers of the fluids.

I shall reserve some further illustrations of this subject for a work which I have long been preparing for the press, and which I flatter myself will contain a clear and simple explanation of the most important parts of natural philosophy. I

have only thought it right, in the present Paper, to lay before the Royal Society, in the shortest possible compass, the particulars of an original investigation, tending to explain some facts and establish some analogies, which have hitherto been obscure and unintelligible.

## IV. *Concerning the State in which the true Sap of Trees is deposited during Winter. In a Letter from* Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P.R.S.*

Read January 24, 1805.

MY DEAR SIR,

It is well known that the fluid, generally called the Sap in trees, ascends in the spring and summer from their roots, and that in the autumn and winter it is not, in any considerable quantity, found in them; and I have observed in a former Paper, that this fluid rises wholly through the alburnum, or sap-wood. But Du Hamel and subsequent naturalists have proved, that trees contain another kind of sap, which they have called the true, or peculiar juice, or sap of the plant. Whence this fluid originates does not appear to have been agreed by naturalists; but I have offered some facts to prove that it is generated by the leaf;* and that it differs from the common aqueous sap owing to changes it has undergone in its circulation through that organ: and I have contended that from this fluid (which Du Hamel has called the *suc propre*, and which I will call the true sap,) the whole substance, which is annually added to the tree, is derived. I shall endeavour in the present Paper to prove that this fluid, in an inspissated state, or some concrete matter deposited by it, exists during

* See Phil. Trans. of 1801, page 336.

the winter in the alburnum, and that from this fluid, or substance, dissolved in the ascending aqueous sap, is derived the matter which enters into the composition of the new leaves in the spring, and thus furnishes those organs, which were not wanted during the winter, but which are essential to the further progress of vegetation.

Few persons at all conversant with timber are ignorant, that the alburnum, or sap-wood of trees, which are felled in the autumn or winter, is much superior in quality to that of other trees of the same species, which are suffered to stand till the spring, or summer: it is at once more firm and tenacious in its texture, and more durable. This superiority in winter-felled wood has been generally attributed to the absence of the sap at that season; but the appearance and qualities of the wood seem more justly to warrant the conclusion, that some substance has been added to, instead of taken from it, and many circumstances induced me to suspect that this substance is generated, and deposited within it, in the preceding summer and autumn.

DU HAMEL has remarked, and is evidently puzzled with the circumstance, that trees perspire more in the month of August, when the leaves are full grown, and when the annual shoots have ceased to elongate, than at any earlier period; and we cannot suppose the powers of vegetation to be thus actively employed, but in the execution of some very important operation. Bulbous and tuberous roots are almost wholly generated after the leaves and stems of the plants, to which they belong, have attained their full growth; and I have constantly found, in my practice as a farmer, that the produce of my meadows has been immensely increased when the herbage of the preceding

year had remained to perform its proper office till the end of the autumn, on ground which had been mowed early in the summer. Whence I have been led to imagine, that the leaves, both of trees and herbaceous plants, are alike employed, during the latter part of the summer, in the preparation of matter calculated to afford food to the expanding buds and blossoms of the succeeding spring, and to enter into the composition of new organs of assimilation.

If the preceding hypothesis be well founded, we may expect to find that some change will gradually take place in the qualities of the aqueous sap of trees during its ascent in the spring; and that any given portion of winter-felled wood will at the same time possess a greater degree of specific gravity, and yield a larger quantity of extractive matter, than the same quantity of wood which has been felled in the spring or in the early part of the summer. To ascertain these points I made the experiments, an account of which I have now the honour to lay before you.

As early in the last spring as the sap had risen in the sycamore and birch, I made incisions into the trunks of those trees, some close to the ground, and others at the elevation of seven feet, and I readily obtained from each incision as much sap as I wanted. Ascertaining the specific gravity of the sap of each tree, obtained at the different elevations, I found that of the sap of the sycamore with very little variation, in different trees, to be 1.004 when extracted close to the ground, and 1.008 at the height of seven feet. The sap of the birch was somewhat lighter; but the increase of its specific gravity, at greater elevation, was comparatively the same. When extracted near the ground the sap of both kinds was almost free

from taste; but when obtained at a greater height, it was sensibly sweet. The shortness of the trunks of the sycamore trees, which were the subjects of my experiments, did not permit me to extract the sap at a greater elevation than seven feet, except in one instance, and in that, at twelve feet from the ground, I obtained a very sweet fluid, whose specific gravity was 1.012.

I conceived it probable, that if the sap in the preceding cases derived any considerable portion of its increased specific gravity from matter previously existing in the alburnum, I should find some diminution of its weight, when it had continued to flow some days from the same incision, because the alburnum in the vicinity of that incision would, under such circumstances, have become in some degree exhausted: and on comparing the specific gravity of the sap which had flowed from a recent and an old incision, I found that from the old to be reduced to 1.002, and that from the recent one to remain 1.004, as in the preceding cases, the incision being made close to the ground. Wherever extracted, whether close to the ground, or at some distance from it, the sap always appeared to contain a large portion of air.

In the experiments to discover the variation in the specific gravity of the alburnum of trees at different seasons, some obstacles to the attainment of any very accurate results presented themselves. The wood of different trees of the same species, and growing in the same soil, or that taken from different parts of the same tree, possesses different degrees of solidity; and the weight of every part of the alburnum appears to increase with its age, the external layers being the lightest. The solidity of wood varies also with the greater or

less rapidity of its growth. These sources of error might apparently have been avoided by cutting off, at different seasons, portions of the same trunk or branch: but the wound thus made might, in some degree, have impeded the due progress of the sap in its ascent, and the part below might have been made heavier by the stagnation of the sap, and that above lighter by privation of its proper quantity of nutriment. The most eligible method therefore, which occurred to me, was to select and mark in the winter some of the poles of an oak coppice, where all are of equal age, and where many, of the same size and growing with equal vigour, spring from the same stool. One half of thepoles which I marked and numbered were cut on the 31st of December, 1803, and the remainder on the 15th of the following May, when the leaves were nearly half grown. Proper marks were put to distinguish the winter-felled from the summer-felled poles, the bark being left on all, and all being placed in the same situation to dry.

In the beginning of August I cut off nearly equal portions from a winter and summer-felled pole, which had both grown on the same stool; and both portions were then put in a situation, where, during the seven succeeding weeks, they were kept very warm by a fire. The summer-felled wood was, when put to dry, the most heavy; but it evidently contained much more water than the other, and, partly at least, from this cause, it contracted much more in drying. In the beginning of October both kinds appeared to be perfectly dry, and I then ascertained the specific gravity of the winter-felled wood to be 0.679, and that of the summer-felled wood to be 0.609; after each had been immersed five minutes in water.

This difference of ten *per cent.* was considerably more than

I had anticipated, and it was not till I had suspended and taken off from the balance each portion, at least ten times, that I ceased to believe that some error had occurred in the experiment: and indeed I was not at last satisfied till I had ascertained by means of compasses adapted to the measurement of solids, that the winter-felled pieces of wood were much less than the others which they equalled in weight.

The pieces of wood, which had been the subjects of these experiments, were again put to dry, with other pieces of the same poles, and I yesterday ascertained the specific gravity of both with scarcely any variation in the result. But when I omitted the medulla, and parts adjacent to it, and used the layers of wood which had been more recently formed, I found the specific gravity of the winter-felled wood to be only 0.585, and that of the summer-felled to be 0.588; and trying the same experiment with similar pieces of wood, but taken from poles which had grown on a different stool, the specific gravity of the winter-felled wood was 0.588, and that of the summer-felled 0.584.

It is evident that the whole of the preceding difference in the specific gravity of the winter and summer-felled wood might have arisen from a greater degree of contraction in the former kind, whilst drying: I therefore proceeded to ascertain whether any given portion of it, by weight, would afford a greater quantity of extractive matter, when steeped in water. Having therefore reduced to small fragments 1000 grains of each kind, I poured on each portion six ounces of boiling water; and at the end of twenty-four hours, when the temperature of the water had sunk to 60°, I found that the winter-felled wood had communicated a much deeper colour to the

water in which it had been infused, and had raised its specific gravity to 1.009. The specific gravity of the water in which the summer-felled wood had, in the same manner, been infused was 1.001. The wood in all the preceding cases was taken from the upper parts of the poles, about eight feet from the ground.

Having observed, in the preceding experiments, that the sap of the sycamore became specifically lighter when it had continued to flow during several days from the same incision, I concluded that the alburnum in the vicinity of such incision had been deprived of a larger portion of its concrete or inspissated sap than in other parts of the same tree: and I therefore suspected that I should find similar effects to have been produced by the young annual shoots and leaves; and that any given weight of the alburnum in their vicinity would be found to contain less extractive matter than an equal portion taken from the lower parts of the same pole, where no annual shoots or leaves had been produced.

No information could in this case be derived from the difference in the specific gravity of the wood; because the substance of every tree is most dense and solid in the lower parts of its trunk: and I could on this account judge only from the quantity of extractive matter which equal portions of the two kinds of wood would afford. Having therefore reduced to pieces several equal portions of wood taken from different parts of the same poles, which had been felled in May, I poured on each portion an equal quantity of boiling water, which I suffered to remain twenty hours, as in the preceding experiments: and I then found that in some instances the wood from the lower, and in others that from the upper parts of the poles,

had given to the water the deepest colour and greatest degree of specific gravity; but that all had afforded much extractive matter, though in every instance the quantity yielded was much less than I had, in all cases, found in similar infusions of winter-felled wood.

It appears, therefore, that the reservoir of matter deposited in the alburnum is not wholly exhausted in the succeeding spring: and hence we are able to account for the several successions of leaves and buds which trees are capable of producing when those previously protruded have been destroyed by insects, or other causes; and for the extremely luxuriant shoots, which often spring from the trunks of trees, whose branches have been long in a state of decay.

I have also some reasons to believe that the matter deposited in the alburnum remains unemployed in some cases during several successive years: it does not appear probable that it can be all employed by trees which, after having been transplanted, produce very few leaves, or by those which produce neither blossoms nor fruit. In making experiments in 1802, to ascertain the manner in which the buds of trees are reproduced, I cut off in the winter all the branches of a very large old pear-tree, at a small distance from the trunk; and I pared off, at the same time, the whole of the lifeless external bark. The age of this tree, I have good reasons to believe, somewhat exceeded two centuries: its extremities were generally dead; and it afforded few leaves, and no fruit; and I had long expected every successive year to terminate its existence. After being deprived of its external bark, and of all its buds, no marks of vegetation appeared in the succeeding spring, or early part of the summer: but in the beginning of July

numerous buds penetrated through the bark in every part, many leaves of large size every where appeared, and in the autumn every part was covered with very vigorous shoots exceeding, in the aggregate, two feet in length. The number of leaves which, in this case, sprang at once from the trunk and branches appeared to me greatly to exceed the whole of those, which the tree had born in the three preceding seasons; and I cannot believe that the matter which composed these buds and leaves could have been wholly prepared by the feeble vegetation and scanty foliage of the preceding year.

But whether the substance which is found in the alburnum of winter-felled trees, and which disappears in part in the spring and early part of the summer, be generated in one or in several preceding years, there seem to be strong grounds of probability, that this substance enters into the composition of the leaf: for we have abundant reason to believe that this organ is the principal agent of assimilation; and scarcely any thing can be more contrary to every conclusion we should draw from analogical reasoning and comparison of the vegetable with the animal economy, or in itself more improbable, than that the leaf, or any other organ, should singly prepare and assimilate immediately from the crude aqueous sap, that matter which composes itself.

It has been contended* that the buds themselves contain the nutriment necessary for the minute unfolding leaves: but trees possess a power to reproduce their buds, and the matter necessary to form these buds must evidently be derived from some other source: nor does it appear probable that the young leaves very soon enter on this office: for the experiments of

* THOMSON's Chemistry.

INGENHOUZ prove that their action on the air which surrounds them is very essentially different from that of full grown leaves. It is true that buds in many instances will vegetate, and produce trees, when a very small portion only of alburnum remains attached to them; but the first efforts of vegetation in such buds are much more feeble than in others to which a larger quantity of alburnum is attached, and therefore we have, in this case, no grounds to suppose that the leaves derive their first nutriment from the crude sap.

It is also generally admitted, from the experiments of BONNET and DU HAMEL, which I have repeated with the same result, that in the cotyledons of the seed is deposited a quantity of nutriment for the bud, which every seed contains; and though no vessels can be traced* which lead immediately from the cotyledons to the bud or plumula, it is not difficult to point out a more circuitous passage, which is perfectly similar to that through which I conceive the sap to be carried from the leaves to the buds, in the subsequent growth of the tree; and I am in possession of many facts to prove that seedling trees, in the first stage of their existence, depend entirely on the nutriment afforded by the cotyledons; and that they are greatly injured, and in many instances killed, by being put to vegetate in rich mould.

We have much more decisive evidence that bulbous and tuberous rooted plants contain the matter within themselves which subsequently composes their leaves; for we see them vegetate even in dry rooms, on the approach of spring; and many bulbous rooted plants produce their leaves and flowers with nearly the same vigour by the application of water only,

* HEDWIG.

as they do when growing in the best mould. But the water in this case, provided that it be perfectly pure, probably affords little or no food to the plant, and acts only by dissolving the matter prepared and deposited in the preceding year; and hence the root becomes exhausted and spoiled: and HASSENFRATZ found that the leaves and flowers and roots of such plants afforded no more carbon than he had proved to exist in bulbous roots of the same weight, whose leaves and flowers had never expanded.

As the leaves and flowers of the hyacinth, in the preceding case, derived their matter from the bulb, it appears extremely probable that the blossoms of trees receive their nutriment from the alburnum, particularly as the blossoms of many species precede their leaves: and, as the roots of plants become weakened and apparently exhausted, when they have afforded nutriment to a crop of seed, we may suspect that a tree, which has borne much fruit in one season, becomes in a similar way exhausted, and incapable of affording proper nutriment to a crop in the succeeding year. And I am much inclined to believe that were the wood of a tree in this state accurately weighed, it would be found specifically lighter than that of a similar tree, which had not afforded nutriment to fruit or blossoms, in the preceding year, or years.

If it be admitted that the substance which enters into the composition of the first leaves in the spring is derived from matter which has undergone some previous preparation within the plant, (and I am at a loss to conceive on what grounds this can be denied, in bulbous and tuberous rooted plants at least,) it must also be admitted that the leaves which are generated in the summer derive their substance from a similar source; and this cannot be conceded without a direct admission of the

existence of vegetable circulation, which is denied by so many eminent naturalists. I have not, however, found in their writings a single fact to disprove its existence, nor any great weight in their arguments, except those drawn from two important errors in the admirable works of HALES and DU HAMEL, which I have noticed in a former memoir. I shall therefore proceed to point out the channels, through which I conceive the circulating fluids to pass.

When a seed is deposited in the ground, or otherwise exposed to a proper degree of heat and moisture, and exposure to air, water is absorbed by the cotyledons and the young radicle or root is emitted. At this period, and in every subsequent stage of the growth of the root, it increases in length by the addition of new parts to its apex, or point, and not by any general distension of its vessels and fibres; and the experiments of BONNET and DU HAMEL leave little grounds of doubt, but that the new matter which is added to the point of the root descends from the cotyledons. The first motion therefore of the fluids in plants is downwards, towards the point of the root; and the vessels which appear to carry them, are of the same kind with those which are subsequently found in the bark, where I have, on a former occasion, endeavoured to prove that they execute the same office.

In the last spring I examined almost every day the progressive changes which take place in the radicle emitted by the horse chesnut: I found it, at its first existence, and until it was some weeks old, to be incapable of absorbing coloured infusions, when its point was taken off, and I was totally unable to discover any albumous tubes, through which the sap absorbed from the ground, in the subsequent growth of the tree, ascends: but

when the roots were considerably elongated, alburnous tubes formed; and as soon as they had acquired some degree of firmness in their consistence, they appeared to enter on their office of carrying up the aqueous sap, and the leaves of the plumula then, and not sooner, expanded.

The leaf contains at least three kinds of tubes: the first is what, in a former Paper, I have called the central vessel, through which the aqueous sap appears to be carried, and through which coloured infusions readily pass, from the alburnous tubes into the leaf-stalk. These vessels are always accompanied by spiral tubes, which do not appear to carry any liquid: but there is another vessel which appears to take its origin from the leaf, and which descends down the internal bark, and contains the true or prepared sap. When the leaf has attained its proper growth, it seems to perform precisely the office of the cotyledon; but being exposed to the air, and without the same means to acquire, or the substance to retain moisture, it is fed by the alburnous tubes and central vessels. The true sap now appears to be discharged from the leaf, as it was previously from the cotyledon, into the vessels of the bark, and to be employed in the formation of new alburnous tubes between the base of the leaf and the root. From these alburnous tubes spring other central vessels and spiral tubes, which enter into and possibly give existence to, other leaves; and thus by a repetition of the same process the young tree or annual shoot continues to acquire new parts, which apparently are formed from the ascending aqueous sap.

But it has been proved by DU HAMEL that a fluid, similar to that which is found in the true sap vessels of the bark, exists also in the alburnum, and this fluid is extremely obvious in the fig, and other trees, whose true sap is white, or coloured. The

vessels, which contain this fluid in the alburnum, are in contact with those which carry up the aqueous sap; and it does not appear probable that, in a body so porous as wood, fluids so near each other should remain wholly unmixed. I must therefore conclude that when the true sap has been delivered from the cotyledon or leaf into the returning, or true sap vessels of the bark, one portion of it secretes through the external cellular, or more probably glandular substance of the bark, and generates a new epidermis, where that is to be formed; and that the other portion of it secretes through the internal glandular substance of the bark, where one part of it produces the new layer of wood, and the remainder enters the pores of the wood already formed, and subsequently mingles with the ascending aqueous sap; which thus becomes capable of affording the matter necessary to form new buds and leaves.

It has been proved in the preceding experiments on the ascending sap of the sycamore and birch, that that fluid does not approach the buds and unfolding leaves in the spring, in the state in which it is absorbed from the earth: and therefore we may conclude that the fluid, which enters into, and circulates through the leaves of plants, as the blood through the lungs of animals, consists of a mixture of the true sap or blood of the plant with matter more recently absorbed, and less perfectly assimilated.

It appears probable that the true sap undergoes a considerable change on its mixture with the ascending aqueous sap; for this fluid in the sycamore has been proved to become more sensibly sweet in its progress from the roots in the spring, and the liquid which flows from the wounded bark of the same tree is also sweet; but I have never been able to detect the slightest degree of

sweetness in decoctions of the sycamore wood in winter. I am therefore inclined to believe that the saccharine matter existing in the ascending sap is not immediately, or wholly, derived from the fluid which had circulated through the leaf in the preceding year; but that it is generated by a process similar to that of the germination of seeds, and that the same process is always going forward during the spring and summer, as long as the tree continues to generate new organs. But towards the conclusion of the summer I conceive that the true sap simply accumulates in the alburnum, and thus adds to the specific gravity of winter-felled wood, and increases the quantity of its extractive matter.

I have some reasons to believe that the true sap descends through the alburnum as well as through the bark, and I have been informed that if the bark be taken from the trunks of trees in the spring, and such trees be suffered to grow till the following winter, the alburnum acquires a great degree of hardness and durability. If subsequent experiments prove that the true sap descends through the alburnum, it will be easy to point out the cause why trees continue to vegetate after all communication between the leaves and roots, through the bark, has been intercepted: and why some portion of alburnous matter is in all trees* generated below incisions through the bark.

It was my intention this year to have troubled you with some observations on the reproduction of the buds and roots of trees;

* I have in a former paper stated that the perpendicular shoots of the vine form an exception. I spoke on the authority of numerous experiments; but they had been made late in the summer; and on repeating the same experiments at an earlier period, I found the result in conformity with my experiments on other trees.

but as the subject of the Paper, which I have now the honour to address to you, appeared to be of more importance, I have deferred those observations to a future opportunity; and I shall at present only observe, that I conceive myself to be in possession of facts to prove that both buds and roots originate from the alburnous substance of plants, and not, as is, I believe, generally supposed, from the bark.

I am, &c.

Elton, Dec. 4, 1804. T. ANDREW KNIGHT.

## V. *On the Action of Platina and Mercury upon each other.* By Richard Chenevix, *Esq. F. R. S. M. R. I. A.* &c.

Read January 10, 1805.

Freyberg, June 3d, 1804.

On the 12th of May, 1803, I had the honour of presenting a Paper to the Royal Society, the object of which was to discover the nature of palladium, a substance just then announced to the public as a new simple metal. The experiments which I had made for this purpose led me to conclude that palladium was not what it had been stated to be, but that it was a compound of platina and mercury.

It was natural to suppose that a subject so likely to spread its influence throughout the whole domain of chemistry, and which tended even to the subversion of some of its elements, would awaken the attention of philosophers. We find accordingly, that it has become a subject of enquiry in England, France, and Germany; but the experiments which I had recommended as the least likely to fail, have been found insufficient to insure the principal result; and I have had the mortification to learn that they have been generally unsuccessful. I have even reason to believe that the nature of palladium is still considered by chemists, at least with a very few exceptions, as unascertained; and that the fixation of mercury by platina is by many regarded as visionary.

The first doubts were manifested in England; and Dr.

WOLLASTON very early denied the accuracy of my inquiries. But as he has not published his experiments, I have had no opportunity of discussing them. His opinion, however, must have such weight in the learned world, that I should have neglected a material fact in the history of palladium, if I had not mentioned it in this place.

In France the compound nature of palladium has been more generally credited. When the National Institute was informed of my experiments, a report was ordered to be made upon them, and M. GUYTON was the person appointed for the purpose. He repeated some of the experiments, and produced some of his results. His general conclusion was the same as mine.

Messrs. VAUQUELIN and FOURCROY then undertook the subject, and they were led by it to the confirmation of the recent discovery of Mons. DESCOTILS. The existence of a new metal, which that chemist had found in crude platina, received great sanction from their experiments; and thus the discussion upon palladium has established a fact which will be considered as interesting, but which would be much more so, were we not already overburthened with substances which our present ignorance obliges us to acknowledge as simple.

No sooner were these celebrated chemists convinced of the existence of a new metal in platina, than they concluded that it must play a principal part in the composition of palladium. Shortly after this, in a note to a letter from M. PROUST to M. VAUQUELIN, in which M. PROUST expresses his astonishment concerning all he has read upon palladium, Mess. FOURCROY and VAUQUELIN further declare, as their opinion, that this compound metal does not contain mercury, but is formed of platina

and the new metal. Whether this new substance does or does not play a principal part in the formation of palladium, could not be ascertained at the time my experiments were made, because the new metal itself was not then known. But from all that Mess. FOURCROY and VAUQUELIN have stated, in such of their different memoirs upon this subject as I have seen, the grounds of their supposition have not appeared. May we not refer their opinion, then, to that common propensity of the mind, against which M. FOURCROY has himself warned us with equal justness and eloquence on another occasion, namely, a proneness to be allured by novelty beyond the bounds of rational belief, and to convert principles which are new into principles of universal influence.

Mess. ROSE and GEHLEN * were the first among the German chemists who instituted experiments upon palladium; and M. RICHTER has also published a paper on the same subject.

The first attempt of Mess. ROSE and GEHLEN to form palladium was by the precipitation of a mixed solution of platina and mercury by green sulphate of iron. Their result was precisely that which I had observed when my operations failed altogether, and which of course was the most frequent. This method was repeated twice. The second time the precipitate of platina and mercury was boiled with muriatic acid, in order to free it from iron; but the latter trial was not more successful than the former.

Their third experiment was, what they have called, a repetition of that in which I had obtained palladium by passing a

* *Neues Allgemeines Journal der Chemie herausgegeben von Hermstadt,* KLAPROTH, RICHTER, SCHEERER, TROMSDORF, und GEHLEN. *Ersten bandes fünftes heft.*

current of sulphuretted hydrogen gas through a mixed solution of platina and mercury. Their method was the following. They dissolved one hundred and fifty grains of platina with four hundred and fifty of mercury, and added a solution of hydrosulphuret of potash. They obtained a precipitate which, at first, was black, afterwards gray; but the whole became black by being stirred. To be certain that all the metal was precipitated, they added an excess of sulphuret of potash, and perceived that a part of the precipitate was redissolved. The liquor was then filtered, and to that part of it, which contained the redissolved precipitate, an acid was added. From this process they obtained a yellow precipitate weighing ninety-one grains; and fifty grains of this, exposed to a strong heat, left three-eighths of a grain of platina. They obtained no palladium from that part of the precipitate which had not been redissolved; and the result of the experiment was complete failure.

I shall not make any observation upon the issue of this process, since, in this case, the best conducted is but too liable to be unsuccessful, and that without any apparent fault in the operator. But as it has been given as a repetition of one of mine, it may not be fruitless to examine how far the repetition was exact.

I had passed a current of sulphuretted hydrogen gas through a mixed solution of platina and mercury, by which means they were precipitated together. My object was so intimately to combine sulphur with these metals, that when exposed to heat, they might (if I may be allowed the expression) be in chemical contact with it at the moment of their nascent metallic state; and as a low temperature suffices, as well to reduce those

metals, as to combine palladium with sulphur, I hoped that those effects might be produced before the total dissipation of the mercury. How far my expectation was fulfilled has been stated in my former Paper.

The sulphuretted hydrogen gas which Mess. ROSE and GEHLEN presented to those metals was combined with potash. Now, in the course of docimastic lectures annually delivered by M. VAUQUELIN at the Ecole des Mines in Paris, when he was Professor at that establishment, it was his constant custom to exhibit an experiment to prove that mercury, precipitated from its solution by many of the alkaline and earthy hydrosulphurets, was redissolved by adding an excess of them.

It is moreover well known, that there is a strong affinity between potash and the oxide of platina, and also that when those substances are brought together in solution, a triple salt, but little soluble, is the result. It was to avoid these difficulties that I had employed uncombined sulphuretted hydrogen gas; for the method adopted by Mess. ROSE and GEHLEN appearing to me to be the application of two divellent forces, I presumed that it would produce a separation. The result of their experiment, which, it appears from their paper, they had not anticipated, shews the necessity of the precaution I had used. The operation which they performed to unite platina and mercury was, in fact, nearly the reverse of that which they supposed they had repeated from me, and might have been applied perhaps with a better prospect of success towards the decomposition of palladium.

Mess. ROSE and GEHLEN seem, in many parts of their paper, to question my having fused platina; and inform us that although they had exposed this metal in the furnace of the

Royal Porcelain Manufactory of Berlin, in which Wedgwood's pyrometer ceased to mark the degree of heat, they could not accomplish its fusion. Many of my friends in England have however seen the buttons which I obtained, and which were not few in number. The flux which I had used was borax. But no mention is made in any one of the operations of Mess. Rose and Gehlen of borax having been employed.

In many of their attempts they obtained an irregular and porous mass, which of course was of a specific gravity much inferior to that of platina; and it might be inferred from their paper that the diminution of specific gravity, which I had observed, was owing to the same cause. It is true, not only that I had very often obtained such a mass, but that I had frequently also observed no diminution whatsoever in the specific gravity of the button which resulted from my operations. But all those upon which I had founded the conclusions alluded to by Mess. Rose and Gehlen were performed in the following manner, and have been repeated since. A Hessian crucible was filled with lamp-black, and the contents pressed hard together. The lamp-black was then hollowed out to the shape of the crucible as far as one-third from the bottom, leaving that much filled with the compressed materials; this lining, which adhered strongly to the sides of the crucible, was made extremely thin in order not to obstruct the passage of caloric. A cylindrical piece of wood, as a pencil, was then forced into the centre of the thick mass of lamp-black at the bottom, and the diameter of this rod was determined by the quantity of metal to be fused, or varied according to other circumstances at pleasure. In general the axis of the cylindrical hole was about three or four times the diameter of the basis. After withdrawing the

rod the crucible was about half filled with borax. Upon this was placed the metal to be fused; and if it had been before melted into a cylindrical form, the axis of the metallic cylinder was placed horizontally, and was of course perpendicular to the axis of the cylindrical excavation at the bottom of the crucible. More borax was then added to cover the piece of metal, and another quantity of lamp-black was pressed hard over the whole in order to keep it tight together. An earthen cover was finally luted to the crucible, and in this state it was exposed to heat in a forge, in which upon another occasion, I had, in the presence of Mess. HATCHETT, HOWARD, DAVY, and others, completely melted a Hessian crucible lined and prepared in the same manner. The fuel which I used was the patent coak of Mess. DAVEY and SAWYER. In the present experiments I moderated the heat so as not materially to injure the crucible, and upon taking it out of the fire, the lining was generally found so compact and so firm that it remained in a solid mass after the crucible was broken. When the metallic cylinder occupied the space at the bottom, it was natural to suppose that it had been fused; because in no other state but that of liquidity could it have run into the mould. In order however to prevent all objections I had the precaution to make the hole of a different diameter from the metallic cylinder, and to observe whether the necessary change in the shape of the latter ensued. If, after such a test, repeated as often as required, I perceived that the metal did not vary in its specific gravity, I thought myself authorised to conclude that it was exempt from air.

M. RICHTER says that he had hoped to have put himself in possession of a considerable piece of palladium by repeating

with minute accuracy the process which I had recommended as the best. He precipitated a mixed solution of platina and mercury by a solution of green sulphate of iron; and after varying the subsequent operations, to which he submitted the product he had obtained by this method, he was led to the following important conclusions amongst others of less consequence. 1st, That two metals, the separate solutions of which are not acted upon by a third body, may be acted upon, and even reduced to the metallic state, by that same body when presented to them in one and the same solution.

2dly. That mercury is capable of entering into combination with platina so, that it cannot afterwards be separated by fire. From the first of these conclusions it is evident, that metals in their metallic state are not incapable of chemical action upon each other; and from the second, that mercury can be fixed (it is purposely that I use the alchemical expression) by platina.

In addition to the chemists abovementioned, I must name two more who in Germany have been occupied by palladium. M. Tromsdorff, in a letter to the authors of the journal already quoted, mentions his having made some fruitless attempts to form this combination; and M. Klaproth, in a letter to M. Vauquelin published in the *Annales de Chimie*, for Ventose, an 12, likewise says that he could not succeed in producing palladium.

Mess. Rose and Gehlen, as well as M. Richter, had conceived from my Paper a reliance on the success of their experiments, which no words of mine had authorised, and have accused me of enforcing the truth of my results with a degree of certainty which their observations do not countenance.

M. Richter supposed that the formation of palladium was attended with no difficulty; and in general they have laid so much stress upon this charge, that I should be inclined to think my Paper had not been read by these chemists. In referring to it again, I find there is hardly a page in which I do not mention some failure, and no experiment, of the very few which occasionally succeeded, is related without my stating at the same time that it was repeatedly unsuccessful. As far as regards palladium, it is rather a narration of fruitless attempts than a description of an infallible process, and more likely to create aversion to the pursuit than to inspire a confidence of success. The course of experiments which I had made, as well before as after reading my Paper to the Society, took me up more than two months, and employed me from twelve to sixteen hours almost every day. I had frequently seven or eight operations in the forge to perform daily, and I do not exaggerate the number of attempts I made during this time, as well in the dry as in the humid way, in stating them to have been one thousand. Amongst these I had four successful operations. I persevered, because even in my failures I saw sufficient to convince me that I should quit the road to truth if I desisted. After all my labour and fatigue I cannot say that I had come nearer to my object, of obtaining more certainty in my processes. Their success was still a hazard on the dice, against which there were many chances; but till others had thrown as often as I had done, they had no solid right to deny the existence of such a combination. On this foundation none, I believe, have established such a right. Mess. Rose and Gehlen do not say how often their experiments were repeated; but it is probable that if they had been

performed very often, these authors would not have neglected to mention it. M. RICHTER states his merely as preparatory to more extensive researches; and M. TROMSDORFF, as well as M. KLAPROTH, mention little more than the fact. If the German chemists have concluded against my results, they have done so without just grounds, and without having bestowed upon them that labour and assiduity for which they are usually so remarkable.

In this state of uncertainty the compound nature of palladium received an indirect, but a very able, support from some experiments of M. RITTER, the celebrated GALVANIST of Jena. M. RITTER had ascertained the rank which a great number of substances hold in a GALVANIC series, arranged according to the property they possess of becoming positive or negative when in contact with each other. He had established the following order, the preceding substance being in a *minus* relation to that which comes next. Zinc, lead, tin, iron, bismuth, cobalt, antimony, platina, gold, mercury, silver, coal, galena, crystallized tin ore, kupfer nickel, sulphur pyrites, copper pyrites, arsenical pyrites, graphite, crystallized oxide of manganese. He had the goodness to try palladium in my presence, and found it to be removed, not only from what I believed to be its constituent parts, but altogether from among the metals, and to stand between arsenical pyrites and graphite. This result led M. RITTER into a new and general train of reasoning, and induced him to undertake the examination of a great number of alloys, and of a variety of amalgams. He considered the subject as a philosopher; and his operations were those of a consummate experimentalist. It would be doing him an injustice to attempt an extract of his ingenious

paper, which contains a series of the most interesting experiments. I shall merely observe for the present purpose, that it very rarely happened that the mixture of two metals bore any determinate relation to the same metals when separate; that in every case the smallest variation in the proportions produced the most marked effects; and that M. RITTER has furnished us with an instrument calculated to detect the presence of such small quantities as have hitherto been considered as out of the reach of chemistry. As palladium presents a very striking instance of the anomaly, to which all compounds seem to be more or less subject, by being removed altogether from the series of simple metals, this may serve to support the other proofs of its compound nature.

One of the principal objections of those who dispute the truth of my conclusions with respect to palladium, is grounded upon the repeated failure of all the methods I had made use of in forming it; but this cannot be of very great weight, when we consider the uncertainty of many other operations of chemistry. The most simple are sometimes liable to fail: and the easiest analyses have often given different products in the hands of different chemists, who yet enjoy indisputable and equal rights to the title of accuracy. The progress which we have made in some parts of the science has not removed the obstacles which impede our advancement in others. We have no method of proving the truth of an experiment except by repeating it: yet this often tends to show nothing more than contradictory results, and consequently the fallibility of the art.

But a recent case has occurred which is perfectly analogous to that of palladium. A few years ago Professor LAMPADIUS,

in distilling some substances which contained sulphur and charcoal, obtained a liquid product of a peculiar nature. He repeated his experiments, but in vain: and after many fruitless attempts abandoned his researches, and confined himself to stating the fact to the chemical world. Little notice was taken of it, and not much interest was excited by an experiment so likely to fail. Some time after this Mess. CLEMENT and DESORMES obtained the same result, and attempted to produce the substance a second time. They performed a vast number of experiments; but their success bore no proportion to their diligence and zeal. They published an account of their process and its consequences, but gained little credit, as no person was fortunate enough to produce the same substance. Many disbelieved the experiments altogether, and denied the existence of such a combination; whilst others, less inclined to doubt, attributed its formation to fortuitous circumstances which might never again occur together. In February, 1804, Professor LAMPADIUS, in distilling some pyritized wood, though with a different intent, obtained the same substance. As he had it now in his power to observe the phenomena that attended its formation, he discovered, and has communicated to the world, a method of producing it, which never fails. Since his late paper upon the subject, as the necessary precautions can be followed by every chemist, Mess. CLEMENT and DESORMES have obtained that credit to which their experiments had, in truth, always been entitled; and the formation, of what Professor LAMPADIUS terms his sulphur-alcohol is no longer a result of chance, or accounted for by being supposed one of those subterfuges to which human pride resorts, in order to spare itself the confession of human weakness.

The observation of any new fact becomes a matter of general concern, and truly worthy of philosophic contemplation, then only, when its influence is likely to be extended beyond the single instance to which it owes its discovery. Whether water were a simple body or a compound could have been of little importance as an insulated fact; but, connected with the vast chain of reasoning it gave rise to, it opened a new field for genius to explore. If in the present case our researches were to be confined merely to ascertaining whether palladium were a simple metal or a compound, all the advantages likely to arise from the facts observed during the inquiry would be lost; and an object of the most comprehensive interest would thus sink into a controversy concerning the existence of one more of those substances, which we have dignified with the name of elements. It was in this point of view that Mess. RICHTER and RITTER considered the subject as far as they went, and a few facts are stated in my first Paper in support of the opinion, that palladium is but a particular instance of a general truth.

By taking the reasoning on this subject then, in its widest extent, we shall be led, I think, to the following conclusion: That metals may exercise an action upon each other, even in their metallic state, capable of so altering some of their principal properties as to render the presence of one or more of them not to be detected by the usual methods. In this is contained the possibility of a compound metal appearing to be simple; but to prove this must be a work of great time and perseverance; and can only be done by considering singly and successively the different cases which it contains, and by instituting experiments upon each. When an affinity which unites two bodies, and so blends their different properties as to

make them apparently one, has taken its full effect, it will not be easy to separate them; and this will be more particularly the case when neither of those substances is remarkable for exercising a powerful action upon others. The method of analysis therefore does not promise much success; and the labour of synthesis is sufficient to deter any individual from the undertaking.

It is my intention now to exhibit one example of my position, and to prove that platina and mercury act upon each other, in such a manner as to disguise the properties of both. I shall therefore wave for the present all consideration of palladium, which is in fact but a subordinate instance of the case before us.

When a solution of green sulphate of iron is poured into a solution of platina, no precipitate, nor any other sensible change ensues. This I had already observed, and it has since been confirmed by all who have written upon the subject. But, if a solution of silver or of mercury be added, a copious precipitate takes place. This precipitate contains metallic platina and metallic silver or mercury; some muriate of one or other of the latter metals is also present, as it is not easy to free the solution of platina from all superfluous muriatic acid. But these salts are of no importance in the experiment, and can be separated by such methods as a knowledge of their chemical properties will easily suggest. The proper object of consideration is the reduction of the platina to the metallic state, which does not happen when it is alone. I have tried to produce the same effect with other metals and platina, but I have not observed any thing similar. It is therefore fair to conclude, that when a solution of platina is precipitated in a metallic state

by a solution of green sulphate of iron, either silver or mercury is present.

The precipitation of a mixed solution of platina and silver requires no further caution than to free the salt of platina as much as possible from muriatic acid; for as I observed in my former Paper, the effect of nitrate of silver poured into muriate of platina, is to produce a precipitate, not of muriate of silver, but of a triple muriate of platina and silver. It was by this experiment that I then proved the affinity of these two metals; for when silver is not present, muriate of platina is among the most soluble salts. The best method of presenting the three solutions of platina, silver, and green sulphate of iron to each other, is first to pour the filtered solution of the last into the solution of platina, and then, after mixing them thoroughly together, to add the solution of silver by degrees, and to stir them constantly. In this, as in all similar operations, the presence of all acids, salts, &c. excepting those necessary for the operation, should be avoided; and if proper proportions have been used, and all circumstances attended to, the precipitation of these two metals will be very complete.

But the precipitation by a solution of mercury requires to be further considered, as the state of oxidizement of this metal, as well as the acid in which it is dissolved, produces a considerable modification in the result. In the first place the oxide, at the minimum of oxidizement, dissolved in muriatic acid, is unfit for the experiment; and even the red oxide dissolved in the same acid, or corrosive sublimate, is not the most advantageous. When a warm solution of the latter is poured into a mixed solution of platina and green sulphate of iron also

warm, as in the case of silver, these substances are brought into contact under the most favourable circumstances. Yet even thus the precipitation is slowly and imperfectly formed, often not till several hours have elapsed; and sometimes a very great deficiency of weight is observed, between the quantities used and those recovered directly by this method. If a solution of nitrate of mercury be used, the effect is produced more rapidly, and the precipitate is more abundant. The precipitation of muriate of platina by nitrate of silver, and the combination which ensues from it, suggested to me an experiment which I must state at length, as from the result of it consequences are deduced which modify some of the experiments of my former Paper.

It occurred to me, that a method of uniting platina and mercury without the intervention of any other metal, or of any substance but the solvents of these metals might be accomplished as in the case of silver and platina. I therefore poured a solution of nitrate of mercury, which solution being at the minimum of oxidizement, consequently formed an insoluble muriate with muriatic acid, into a solution of muriate of platina. The result was a triple salt of platina and mercury, which when the mercury was completely and totally at the minimum of oxidizement was nearly insoluble. To procure it in this state it is sufficient to put more metallic mercury into dilute nitric acid than the nitric acid can dissolve, and to boil them together. This triple salt of platina and mercury shall be presently examined. From this it is evident that to produce the union of platina and mercury, the latter being at its minimum of oxidizement in nitric acid the addition of green sulphate of iron is superfluous.

But if mercury be raised to its maximum of oxidizement in nitric acid the case is different, for no precipitation occurs till the green sulphate of iron is added. The most advantageous method for precipitating platina and mercury by green sulphate of iron is, I believe, the following. Mix a solution of platina with a solution of green sulphate of iron, both warm, and add to them a solution of nitrate of mercury at the maximum of oxidizement also warm. It is necessary to avoid excess of acid, salt, &c. in this as in all such cases. With due care the precipitation of both metals will then be complete.

By comparing the experiments made with mercury and platina with those made with silver and platina, a striking resemblance will be found. This induced me to pursue the analogy, and to examine whether, independently of the action of platina, mercury had not the same property of being precipitated by green sulphate of iron as silver. Nitrate of silver is precipitated by green sulphate of iron, but muriate of silver is not sensibly acted upon by the same reagent. The insolubility of muriate of silver might be alleged as the cause of this, if I had not tried the experiment by pouring nitrate of silver into green muriate of iron, in which case all the substances were presented to each other in solution. The result was not reduction, but muriate of silver and nitrate of iron. This fact rests upon a much more extensive basis than mere mechanical circumstances; and, if pursued with the attention it deserves, it would lead us into the wide expanse of complicated affinities and their relations. From reasoning alone we should be disposed to think that an acid, so easily decomposed as the nitric, would be sufficient to prevent the reduction of a metal which it can dissolve. But on the one hand it can spend its oxygen

upon a part of the oxide of the green sulphate of iron, while on the other its affinity for oxide of silver is not powerful enough to retain it, when there is another part of the oxide of iron present to deprive it of oxygen. But the affinity of muriatic acid for oxide of silver, one of the strongest at present known, is sufficient to counterbalance all the other forces. There are many other instances of the same kind.

If then a solution of green sulphate of iron be brought into contact with either soluble or insoluble muriate of mercury, no reduction takes place; but if mercury, whether at the maximum or the minimum of oxidizement, be dissolved in nitric acid, and green sulphate of iron be added, the mercury is precipitated in the metallic state.

These experiments are much stronger examples than the former of the effects produced by complicated affinities. They are of importance not only as objects of general consideration but in their application to the present subject. They most materially modify and are indispensable to the accuracy of the results I formerly stated; but I was not aware of them at the time I first engaged in the investigation of this subject. I can also now explain a very material difference between some proportions observed by M. Richter and myself in an experiment which that chemist had made as a repetition of one of mine.

I had poured a solution of green sulphate of iron into a solution of 100 parts of gold and 1900 of mercury, and had obtained a precipitate consisting of 100 of gold and 774 of mercury. M. Richter repeated, as he terms it, this experiment; that is, he used 100 of gold and 300 of mercury, and

obtained a precipitate weighing 108. He is surprised at the difference of weight between our results, which might be owing to his *method* of *repeating* the experiment; but the real cause of this difference lies, as I suppose, in my having accidentally used nitrate instead of muriate of mercury. I had never observed that with mercury and silver this operation had failed, and it must have been, because, on account of the known effect of muriatic salts upon those of silver, I had naturally avoided using a muriate of mercury.

But the state of the nitrate of mercury which is used with a solution of gold is not indifferent. As green sulphate of iron reduces mercury when dissolved in nitric acid, as well as gold, it is necessary to mix the solutions of those metals before the green sulphate of iron is added, in order that both may be acted upon together. If the nitrate be at the minimum of oxidizement, a precipitate is immediately formed upon mixing the solutions of gold and mercury. Calomel is produced by the muriatic acid of the solution of gold and the oxide of mercury; whilst the gold is reduced to the metallic state by a portion of the oxide of mercury becoming more oxidized, and forming the soluble muriate. The precipitate consists of calomel, of metallic gold, and of a very small portion of mercury which I believe to be in the same state; my reason for thinking so, is, that I have often observed, that a glass vessel in which I had sublimed some of it, was lined with a thin gray metallic coat. If, on the contrary, a nitrate of mercury be highly oxidized, no precipitate nor reduction of gold takes place until the green sulphate of iron is added. But at any rate the precipitation of gold and mercury, or of silver and mercury by green sulphate

of iron cannot be adduced as an argument to support the affinity of these metals, since the effect is the same, whether they are separate or united.

These preliminary considerations were necessary as well for the rectification of my former experiments as for the pursuit of my present object; and now to return to platina.

*Exper.* 1. If a solution of highly oxidized nitrate of mercury be poured into a mixed solution of platina and green sulphate of iron, the first action which takes place passes between the muriatic acid of the solution of platina and the oxide of mercury, by which a muriate of mercury is formed, but retained in solution. This effect makes it advantageous to use a greater quantity of the solution of mercury than is merely capable of drawing down the given quantity of platina along with itself in the form of a metallic precipitate. When this precipitate is washed and dried, it will be found to weigh much more than the original quantity of platina; and the augmentation of weight has no limit but those of the mercury and the green sulphate of iron employed. But even after nitric acid has been boiled for a long time and in great quantities upon this precipitate, until it no longer dissolves any part of it, there still remains more undissolved matter than the original weight of the platina used in the experiment. By exposure to heat little more is left in general than the original platina; and sometimes even a diminution may be observed; for as the experiment is not attended with uniform success, it does not always happen that the whole of the platina is precipitated, but a portion of it will sometimes resist the action of the green sulphate of iron, even when sufficient mercury has been used. Before the precipitate has been exposed to heat it is dissolved

more easily than platina by nitro-muriatic acid; and the solution when nearly in a neutral state gives a copious metallic precipitate, (yet not equal to the quantity employed,) when boiled with a solution of green sulphate of iron.

*Exper.* 2. When a mixed solution of platina and mercury is precipitated by metallic iron, a quantity equal to the sum of the former metals is generally obtained. After nitric acid has been boiled for a long time upon the precipitate so formed, the original weight of platina, together with a considerable increase, remains behind, nor can nitric acid sensibly diminish it. It yields more easily than platina to the action of nitro-muriatic acid, and its solution in that acid, when neutralized, gives a precipitate, as in the former experiment, by green sulphate of iron. If this precipitate be exposed to a strong heat after it has been boiled with nitric acid, it loses a great part of its weight, and the platina alone will generally be found to remain.

*Exper.* 3. When a quantity of ammoniacal muriate of platina is treated according to the method of Count MUSSIN PUSHKIN to form an amalgam, and, after being rubbed for a considerable time with mercury, is exposed in a crucible to a heat gradually increased till it becomes violent, a metallic powder remains in the crucible. This powder is acted upon by nitro-muriatic acid, and when the solution is neutralized, a copious precipitate is formed upon the addition of green sulphate of iron. This effect takes place even after the metal has been fused in the manner described in the former part of this Paper.

*Exper.* 4. If sulphur be added to the ingredients recommended by Count MUSSIN PUSHKIN, and the whole treated as in the last experiment, the quantity of precipitate caused by green sulphate of iron in the nitro-muriatic solution of the

button which results from the operation, is generally more considerable.

*Exper.* 5. If sulphur be rubbed for some time with ammoniacal muriate of platina, and the mixture be introduced into a small Florence flask, it can be melted on a sand-bath. If mercury be then thrown into it, and the whole be well stirred together and heated, it may afterwards be exposed to a very strong fire and melted into a button. If this be dissolved in nitro-muriatic acid, it will give a precipitate, as in the former cases, by green sulphate of iron.

*Exper.* 6. If a current of sulphuretted hydrogen gas be sent through a mixed solution of platina and mercury, and the precipitate which ensues be collected, the metal may be reduced by heat; and with the addition of borax, it may be melted into a button which will not contain any sulphur. Green sulphate of iron causes a precipitate in the solution of this metal also.

*Exper.* 7. If to a mixed solution of platina and mercury, phosphate of ammonia be added, a precipitate takes place. If this be collected and reduced, it will be acted upon by green sulphate of iron poured into its solution, in the same manner as the metallic buttons in the preceding examples.

*Exper.* 8. I have already mentioned that when a solution of nitrate of mercury, at the minimum of oxidizement, is poured into a solution of muriate of platina, a mercurial muriate of platina is precipitated. The supernatant liquor may be decanted and the residuum washed; if this be reduced and afterwards dissolved in nitro-muriatic acid, it will yield a precipitate with green sulphate of iron. This method appears to me to be the neatest for combining platina and mercury, as the action which takes

place is independent of every substance except the metals themselves.

*Exper.* 9. One of the most delicate tests that I have observed in chemistry is recent muriate of tin, which detects the presence of the smallest portion of mercury. When a single drop of a saturate solution of neutralized nitrate or muriate of mercury is put into 500 grains of water, and a few drops of a saturate solution of recent muriate of tin are added, the liquor becomes a little turbid, and of a smoke-gray colour. If these 500 grains of liquid be diluted with ten times their weight of water, the effect is of course diminished, but still it is perceptible. I had on a former occasion observed the action of recent muriate of tin upon a solution of platina. If a solution of recent muriate of tin be poured into a mixed solution of platina and mercury, not too concentrated, it can hardly be distinguished from a simple solution of platina. But if too much mercury be present the excess is acted upon as mercury; and the liquor assumes a darker colour than with platina alone.

From all these experiments it is evident that mercury can act upon platina, and confer upon it the property of being precipitated in a metallic state by green sulphate of iron. By *Experiments* 1 and 2, it is proved, 1st, That platina can protect a considerable quantity of mercury from the action of nitric acid; and 2dly, That mercury can increase the action of nitro-muriatic acid upon platina. From *Experiments* 3, 4, 5, 6, 7, 8, it appears that mercury can combine with platina in such a manner as not to be separated by the degree of heat necessary to fuse the compound, since after the fusion it retains that property, which is essentially characteristic of the presence of mercury in a solution of platina. The 8th *Experiment* proves

that the action of mercury upon platina is not confined to the metallic state; but that these metals can combine and form an insoluble triple salt with an acid which produces a very soluble compound with platina alone. The 9th *Experiment* shows that platina can retain in solution a certain quantity of mercury, and prevent its reduction by a substance which acts most powerfully to that effect, when platina is not present. That part of the general position therefore which is the object of this Paper is proved, if these experiments, upon being repeated by other chemists, shall be found to be accurate.

One or two of the above experiments seem to be in contradiction to some that I have stated in my Paper upon palladium; for in the present examples platina protects mercury against the action of nitric acid; whereas in palladium the mercury is not only acted upon itself, but it conduces to the solution of platina in the same acid. I am well aware of this objection; but confining myself to my present object, I shall wave all further discussion of it till another opportunity. In the mean time, however, it may be laid down as an axiom in chemistry, that the strongest affinities are those, which produce in any substance the greatest deviation from its usual properties.

When a button of the alloy of platina and mercury as prepared by any of the above methods, is dissolved in nitro-muriatic acid, and afterwards precipitated by green sulphate of iron, the entire quantity of the alloy used is seldom obtained. A considerable portion of platina resists the action of green sulphate of iron, and remains in solution. This may be looked upon as the excess of platina, and can be recovered by a plate of iron. Hence it appears that less mercury is fixed, than can determine the precipitation of the entire quantity of platina;

yet in this state it can draw down a greater quantity of the latter, than when it is merely poured into a mixed solution of platina, not before so treated. Indeed the whole of these experiments tend, not only to show that these two metals exercise a very powerful action upon each other, but that they are capable of great variation in the state of their combination; and also that substances possessing different properties have resulted from my attempts to combine platina with mercury.

This observation furnished me with a method of ascertaining, or at least of approaching to the knowledge of, the quantity of mercury thus fixed by platina, and in combination with it. The experiment, however, having been seldom attended with full success, I mention the result with the entire consciousness of the uncertainty to which it is subject. I observed the increase of weight, which the original quantity of platina had acquired in some cases after it had been treated with mercury, and fused into a button. I counted that augmentation as the quantity of mercury fixed. I then determined how much was precipitated by green sulphate of iron from a solution of this alloy, and supposed it to contain the whole quantity of mercury found as above. But, even if attended with complete success, there is a chemical reason which must make us refuse our assent to this estimate. It is possible, and not unlikely, that a portion of mercury may be retained in solution by the platina, as well as that a portion of the platina may be precipitated by means of the mercury. The mean result, however, was that the precipitate by green sulphate of iron consisted of about 17 of mercury, and 83 of platina, when the specific gravity was about 16.

With regard to palladium, lest it should be supposed that

either my own observations, or those of others have given me cause to alter my opinion. I will add that I have as yet seen no arguments of sufficient weight to convince me, in opposition to experiment, that palladium is a simple substance. Repeated failure in the attempt to form it I am too well accustomed to, not to believe that it may happen in well conducted operations; but four successful trials, which were not performed in secret, are in my mind a sufficient answer to that objection. By determining the present question we may overcome the prepossession conceived by many against the possibility of rendering mercury as fixed, at an elevated temperature, as other metals: we may be led to see no greater miracle in this compound than in a metallic oxide, or in water, and be compelled to take a middle path between the visions of alchemy on the one hand, and the equally unphilosophical prejudices on the other, which they are likely to create. In the course of experiments just now related, I have seen nothing but what tends to confirm my former results, yet the only means which I can, after all, prescribe for succeeding, is perseverance.

To ascertain whether the opinion of Mess. Fourcroy and Vauquelin, that the new metal was the principal ingredient in palladium had any just foundation, I observed the methods they have recommended for obtaining pure platina; but I did not perceive any difference in the facility with which either kind of platina combined with mercury.

I might have added some more experiments to corroborate the evidence I have adduced to prove my assertion of the fixation of mercury by platina; but Mess. Vauquelin and Fourcroy have promised the Institute of France a continuation of their researches, and M. Richter concludes his paper with

saying that he will return to the subject. From the labours of such persons some great and important fact must issue, and I hope that the present subject will not be excluded from their consideration. The facts contained in this Paper cannot be submitted to too severe a scrutiny; and no judge can be more rigid or more competent than the very person who was the first to doubt my former experiments. But it is necessary to be observed by whoever shall think them worth the trouble of verifying, that even these experiments are liable to fail unless proper precautions are used: that I have never operated upon less than one hundred grains; and that the results, which I have stated, however simple they may appear, have been the constant labour of some weeks.

## POSTSCRIPT.

Since this Paper was written Dr. WOLLASTON has published some experiments upon platina. He has found that palladium is contained in very small quantities in crude platina. This fact was mentioned to me more than a year ago by Dr. WOLLASTON. I have not yet seen a copy of his Paper; but I shall merely observe here that, whatever be the quantity of palladium found in a natural state, no conclusion can be drawn as to its being simple or compound. Nothing is more probable than that nature may have formed this alloy, and formed it much better than we can do. At all events the amalgamation to which platina is submitted before it reaches Europe is sufficient to account for a small portion of palladium.

## VI. *An Investigation of all the Changes of the variable Star in* Sobieski's *Shield, from five Year's Observations, exhibiting its proportional illuminated Parts, and its Irregularities of Rotation; with Conjectures respecting unenlightened heavenly Bodies.* By Edward Pigott, *Esq.* *In a Letter to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read February 7, 1805.

Bath, 1802.

THE object of the first part of this Paper is a further investigation of the periodical and other changes of brightness of one of the variable stars I discovered in 1795, that in SOBIESKI's *shield*, an account of which the Royal Society did me the honour of publishing in their Transactions. Those determinations being deduced from a few periods made *near the time of discovery*, must of course remain unsatisfactory, however exact the observations themselves may be, until *confirmed* by an additional set, or by others made at a greater interval of time; for which purpose I occasionally continued keeping a journal of its changes for near five years, and am happy to find that they have answered my expectation, particularly by giving us an insight into its irregularities, as will be shewn hereafter.

*Variable Star in* SOBIESKI's *Shield.*

R. A. - - $279^{\circ}\ 9\frac{1}{2}'$
S. declination $5^{\circ}\ 56'$ } for the end of June, 1796.

Its rotation on its axis was, in 1796, estimated at 62¼ days, from a mean of six observations of its greatest and least brightness. Here follow about 26 similar determinations, most of them the results of very accurate observations; and as they probably will in future be compared with others, I have examined them repeatedly with the utmost care, attending particularly to the progression of their changes.

Table I.

| Dates when at its greatest Brightness. | Magnitudes. | Dates, when at its least Brightness. | Magnitudes. |
|---|---|---|---|
| 1796. September 17 | 5 | 1796. September 3 | 6 |
| November 13 | 5— | October 22 - | 6 |
| 1797. May 14: - | 5+ | 1797. July 10 - - | 5.6 |
| August 7 - | 5 | September 15 | 6 |
| October 15 - | 6.5 | November 6 | 6 |
| 1798. July 29 - | 5+ | 1798. July 10 - - | 6 |
| October 25 - | 5.6 | September 15 | 9.0 |
| December 5:: | 5.6 | November 10 | 6 |
| 1799. June 1:: - | 6.5 | 1799. July 4 - | 7 |
| August 7 - | 5 | September 16 | 6 |
| October 11 - | 5+ | November 5: | 6.7 |
| 1801. July 14: - | 5 | 1801. June (middle): | 6 |
| September 24 | 5 | August 21 - | 6.7 |
| | | October 16 - | 6.5 |

The + and — annexed to the magnitudes denote them to be more or less bright; the doubtful results are marked with dots; all the others are esteemed exact, except those of August 7, 1797, and August 21, 1801, which are in *a small degree* less so. From these determinations the rotation on its axis may be computed as follows.

## Table II.

*Middle of its greatest Brightness.*

| Dates. | | Interval in Days. | | | Number of Periods. |
|---|---|---|---|---|---|
| 1796. | September 17<br>November 13 | 57 | - | equal to - | 1 |
| 1797. | May 14 :<br>August 7 - | 85 | - | = - | 1 |
| | August 7 -<br>October 15 | 69 | - | = | 1 |
| 1798. | July 29 -<br>October 25 | 88 | - | = - | 1 |
| 1799. | August 7 -<br>October 11 | 65 | - | = | 1 |
| 1796.<br>1797. | November 13<br>May 14 : - | 182<br>or 61 — | - | =<br>= | 3<br>1 |
| 1796.<br>1797. | November 13<br>August 7 - | 267<br>or 67 — | - | =<br>= | 4<br>1 |
| 1797.<br>1798. | October 15<br>July 29 - - | 287<br>or 57½ | - | =<br>= | 5<br>1 |
| 1798.<br>1799. | October 25<br>August 7 - | 286<br>or 57 + | - | =<br>= | 5<br>1 |
| 1801. | July 14 : -<br>September 24 | 72 : | - | = | 1 |

## Table III.

*Middle of its least Brightness.*

| Dates. | Interval in Days. | | Number of Periods. |
|---|---|---|---|
| 1796, September 3 — October 22 | 49 | equal to | 1 |
| 1797, July 10 — September 15 | 67 | = | 1 |
| September 15 — November 6 | 52 | = | 1 |
| 1798, July 10 — September 15 | 67 | = | 1 |
| September 15 — November 10 | 56 | = | 1 |
| 1799, July 4 — September 16 | 74 | = | 1 |
| September 16 — November 5 | 50 | = | 1 |
| 1801, August 21 — October 16 | 56 | = | 1 |
| 1796, October 22 — 1797, July 10 | 261 or 65¼ | = | 4 |
| 1797, November 6 — 1798, July 10 | 246 or 61½ | = | 4 |
| | | = | 1 |
| 1798, November 10 — 1799, July 4 | 236 or 59 | = | 4 |
| | | = | 1 |

From all these results it appears, that the disagreements between them are far greater when at its full brightness than at its least; I shall therefore, in summing up the first set, omit two of them, as they evidently differ considerably from the others.

## Table IV.

| Rotation from Observations of its full Brightness. | Rotation from Observations of its least Brightness. |
|---|---|
| Days. | Days. |
| 57 | 49 |
| 69 | 67 |
| 65 | 52 |
| 61+ | 67 |
| 67− | 74 |
| 57½ | 50 |
| 57+ | 56 |
| 72 | 56 |
| | 65+ |
| | 61½ |
| | 59 |
| by its full brightness 63+ on a mean. | By its least ditto 59½ on a mean. |

A mean of these two means being 61¼ days, agrees with the first deductions to 1¼ day, a coincidence that certainly I could not flatter myself would have happened: yet it must be remembered, that the intervals with considerable perturbations were omitted; for, had they been included, the length of period resulting from its *maxima* of brightness would have varied much more from that obtained from its *minima*. I shall now proceed to examine some of its other changes.

Table V.

| Decrease from the Middle of its full Brightness to the Middle of its least. See Table I. | | | Increase from the Middle of its least Brightness to the Middle of its full. See Table I. | | |
|---|---|---|---|---|---|
| 1796. | September 17 – October 22 | 35 days. | 1796. | September 3 – September 17 | 14 days. |
| 1797. | May 14 – July 10 | 57 | | October 22 – November 18 | 22 |
| | August 7 – September 15 | 39 | 1797. | July 10 – August 7 | 28 |
| | October 15 – November 6 | 22 | | September 15 – October 15 | 30 |
| 1798. | July 29 – September 15 | 48 | 1798. | July 10 – July 29 | 19 |
| | October 25 – November 10 | 16 | | September 15 – October 25 | 40 |
| 1799. | August 7 – September 16 | 40 | 1799. | July 4 – August 7 | 34 |
| | October 11 – November 5 | 25 | | September 16 – October 11 | 25 |
| 1801. | July 14 – August 21 – September 24 – October 16 | 38 22 | 1801. | August 21 – September 24 | 34 |
| | | 34 on a mean. | | | 27+ on a mean. |

The sum of these two means (61+), agreeing so satisfactorily with the whole rotation ($61\frac{1}{2}$), no correction is requisite, as was the case with the former determinations of 1796 to reduce them to 28 and 35 days, results that differ considerably from the above (34 and 27+); but as they were deduced from only *two* intervals, the disagreement cannot be of any consequence, provided the number of each set be *proportionally*

attended to in the computation, and then the mean of the whole will be 33+ and 29— days: thus it appears that the *time of the decrease is longer* than that of the *increase*, and consequently that the places of the full and the least brightness are not situated at the distance of half the circumference from each other: the like circumstance will be found to be the case with most, if not all, of the variable stars. The next particulars to be reviewed are *the durations* of its brightness without *any perceptible change*, while at its *maximum and minimum*. These determinations require a tolerable *succession* of observations: where therefore that is not the case, they are omitted.

Table VI.

| Duration of Brightness at its Maximum. | | | Duration of Brightness at its Minimum. | | |
|---|---|---|---|---|---|
| 1796. | Days. | Magnitudes. | 1796. | Days. | Magnitudes. |
| September 17 | 9 | 5 | September 3 | 7 | 6 |
| November 13 | 8 | 5— | October 22 | 8 | 6 |
| 1797. | | | 1797. | | |
| October 15 | 32 | 6.5 | July 10 | 24 | 5.6 |
| 1798. | | | September 15 | 18: | 6+ |
| July 29 | 6 | 5+ | November 6 | 6 | 6 |
| October 25 | 10 | 5.6 | 1798. | | |
| December 5 | 16 or more | 5.6 | July 10 | 12 | 6 |
| 1799. | | | September 15 | 9 | 9 |
| June 1 | 16:: | 6.5 | November 10 | 8 | 6+ |
| August 7 | 8 | 5 | 1799. | | |
| October 11 | 8 | 5+ | July 4 | 9 | 7 |
| 1801. | | | September 16 | 10 | 6 |
| September 24 | 15:: | 5 | November 5 | 16: | 6.7 |
| | | | 1801. | | |
| | | | October 16 | 9 | 6.5 |

It appears in general by my journal, and from these results, that when *the degree* of brightness at its maximum is *less than usual*, and its minimum *not much decreased*, the changes take place but very slowly, and cannot be settled with much accuracy, unless the observations have been made frequently, and with great attention; therefore, in summing them up, I think four of the first set and three of the second may be omitted, and then the duration at its maximum will be on a mean 8 + days,

and ditto 20— days

when it does not attain its usual brightness;

and at its minimum - - - on a mean 9— days,

and ditto 20— days

when its decrease is not so great as usual; the former observations make them 14 and 9 days.

Some of its *degrees* of brightness annexed to the results, have occasionally been noticed, as far as it was necessary, but the list of them I am going to give, is more exact and full. It will be there seen, that its brightness is seldom the same for two or three successive periods; that the change in half a rotation is sometimes from the 5th to the 7th magnitude, and sometimes only half a one or scarcely perceptible: its decrease has also been greater than by the former observations, particularly on September 15, 1798, and August 9, 1803,* when it was less than the 9th magnitude, or had even disappeared.

* Added since the Paper was written.

Table VII.

| Dates. | Magnitudes when at its full Brightness. | Dates. | Magnitudes when at its least Brightness. |
|---|---|---|---|
| 1796. September 17 | 5 | 1796. September 8 | 6 |
| November 18 | 5 small | October 22 | 6 |
| 1797. May (middle) | 5 bright | 1797. July 10 | 5.6 |
| August 7 | 5 | September 15 | 6 bright |
| October 15 | 6.5 | November 6 | 6 |
| 1798. July 29 | 5 bright | 1798. July 10 | 6 |
| October 25 | 5.6 | September 15 | 6 or 5 |
| December 5 | 5.6 | November 10 | 6 bright |
| 1799. June 1 | 6.5 | 1799. July 4 | 7 |
| August 7 | 5 | September 16 | 6 |
| October 11 | 5 bright | November 5 | 6.7 |
| 1801. July 14 | 5 | 1801. June (middle) | 6 |
| September 24 | 5 | August 21 | 6.7 |
| | | October 16 | 6.5 |
| | | 1803. August 9* | 6 or 5 |

In concluding these determinations I shall collect together, as follows, in one view, all the different changes that have been examined; the first column describes them, the second exhibits the present results, the third the former ones, and the last column a mean of both, computed *proportionally* according to the number of observations of each.

* Added since the Paper was written.

Table VIII.

| | Days. | Days. | Days. on a mean. |
|---|---|---|---|
| Rotation on its axis | 61¼ | 62¼ | 62— |
| Duration of brightness, at its maximum, without any perceptible change | 8+ | 14 | 9⅓ |
| Ditto, when it does not attain its usual brightness | 20— | — | — |
| Duration of brightness at its minimum, without any perceptible change | 9— | 9 | 9 |
| Ditto, when it does not decrease so much as usual | 20— | — | — |
| Decrease in time, from the middle of its full brightness to the middle of its least | 34 | 28 | 33+ |
| Increase in time, from the middle of its least brightness to the middle of its full | 27+ | 35 | 29— |
| Extremes of its different degrees of brightness; with a mean of its usual variations | 5+ 9 or 0 | 5+ 7.8 | 5 6 |

## SECOND PART.

Fontainbleau, 1803.

These essential variations of the star being thus settled with considerable precision, we may proceed to examine some of its other phenomena, particularly one common to most of the variables, as likewise in some degree to our sun, *viz. that the times of their periodical returns of brightness* are, in general, IRREGULAR, a circumstance I apprehend sufficiently interesting to engage our attention, at least I have ever thought so, and was thereby induced a few years past to make a succession of observations on one of them, in hopes of finding in what manner such *irregularities* took place, or at least to leave to future astronomers determinations, that might lead them to form some ultimate opinion thereon. I therefore chose for that purpose the star in SOBIESKI's *shield*, on account of the time of its revolution on its axis being comparatively of a moderate length, *viz.* 6*d* days, and shall here have the honour of laying before the Society the appearances that occurred, point out the various results deduced from the observations, and attempt to explain them. The two following Tables are the observed middle times of its *full* and *least* brightness, with deductions of the star's apparent rotation from *single intervals*, which in the present examination can alone be admitted, because a mean taken of two or several would in general make such irregularities disappear, by the long and the short ones compensating each other. The remarks for the present need not be attended to, as they are chiefly to explain the reliance that may be put on some of the observations.

Table IX.

| The observed middle Times of its *full* Brightness. | Apparent Rotations in Days. | Remarks, chiefly to illustrate some of the Observations. |
|---|---|---|
| 1795. October 1 - | 70 | By the observations of November, &c. it seems probable it had not obtained its full brightness before December 10, although possibly much later. |
| December 10: | | |
| 1796. April 10 - | 69 | |
| June 18 - - | 89 | The increase towards July 27 was so slight that I had much hesitation in adopting it as a full brightness; if omitted, the interval will be 91 days. See Phil Trans. 1797. |
| July 27 - | 52 | |
| September 17 - | 57 | |
| November 13 | | |
| 1797. May 14: - | 85 | The full brightness in May is doubtful to only about 6 days; the observations afterwards, to August the 7th, were made with tolerable regularity. |
| August 7 - | 69 | |
| October 15 - | | |
| 1798. July 29 - | 88 | A regular succession of observations were made between July and October 25. |
| October 25 - | 41 | The last observation made, was on December 10, when it shewed no appearance of decreasing, although it had been 16 days at its full brightness. |
| December 5: | | |
| 1799. June 1: - | 67 | The full brightness lasted a fortnight. |
| August 7 - | 65 | |
| October 11 - | | |
| 1801. July 14: - | 72 | The observation of July 14, is doubtful to a few days, to which perhaps the excess may be attributed. |
| September 24 | | |
| November 1 | 88 | A regular succession of observations were made between September and the middle of November. This last determination was deduced after the first part of this Paper was finished. |

## Table X.

| The observed middle Times of its *least* Brightness. | Apparent Rotations in Days. | Remarks, chiefly to illustrate some of the Observations. |
|---|---|---|
| 1796. March 4 - May 10 - | 67 | |
| May 10 - July 19 - | 70 | The decrease of July 19 being so very slight, I for a long time omitted it, and took the interval from May to September of 116 days as a double revolution, but have here preferred the separate ones of 70 and 46 days. See Phil. Trans. 1797. |
| July 19 - September 3 | 46 | |
| September 3 - October 22 - | 49 | |
| 1797. July 10 - September 15 | 67 | |
| September 15 - November 6 | 52 | |
| 1798. July 10 - September 15 | 67 | |
| September 15 - November 10 | 56 | |
| 1799. July 4 - September 16 | 74 | The increase and decrease observed by a succession of good observations. |
| September 16 - November 5 - | 50 | |
| 1801. Middle of June : : August 21 - October 16 - | 56 | |

It thus appears, that the periodical returns of brightness are uncommonly fluctuating, and that the differences between the extremes are very considerable; to account for which, I shall presume to offer the following explanations,

suggesting previously a few plausible conjectures, and some inferences arising from the observations themselves.

1st. That the body of the stars are dark and solid.

2d. Their real rotations on their axes are regular.

3d. That the surrounding medium is by times generating and absorbing its luminous particles in a manner nearly similar to what has been lately so ingeniously illustrated by the great investigator of the heavens, Dr. HERSCHEL, with regard to the sun's atmosphere.

4th. That these luminous particles are but *sparingly dispersed* in the atmosphere surrounding the variable star of SOBIESKI, appears from the star being occasionally diminished to the 6.7 magnitude, and much less. July 4, 1799, it was of the 7th; September 15, 1798, and August 9, 1803, *of the 9th, if not invisible*. (See Table VII.) Does not this indicate a very small portion of light on its *darkened hemisphere*?

5th. And may we not with much plausibility consider them as spots, somewhat circular, or of no great extent? for even on its *brightest hemisphere* the *duration* of its full lustre is, on a mean, only $9\frac{1}{2}$ days of the 62, or about one-sixth and $\frac{1}{4}$ of its circumference. (See Table VIII. page 140.) The dimensions therefore of the parts enlightened seem much circumscribed, and can be tolerably estimated, and consequently may be represented very small, particularly if the *powerful effect of a little light* and *the length of time* a bright spot is remaining in view be taken into consideration.

6th. And a further ground of presumption that those principal bright parts are but slight patches is, that they undergo *perpetual changes*, and also that such changes are very visible to us, for most probably they would be imperceptible, were not the

bright parts contrasted by considerable intervals or diminutions of light.

7th, and last. We may obtain some idea of the *relative situation or intervals between* these bright parts, by the observations of the increase and decrease of brightness, as thereby the changes and times elapsed are pointed out. (See Table V. page 136; and Phil. Trans. for 1797.)

I have tried practically the effect of the above suppositions, by placing small white spots on a dark sphere, which being revolved round represented the various changes as nearly as could be expected: proceeding therefore with these and other considerations, I shall make ideal drawings of the star with the small illuminated parts in its atmosphere, and apply to them some of the actual observations from both the preceding Tables, having always in view that each period may, more or less, require a different disposition of spots, in consequence of their constant changeability.

*1st View.*

Plate II. Fig. 1, A B, the star's polar axis, round which its rotation takes place in 62 days from C to D.

CD, its equator, the 360 degrees of which being revolved in 62 days, gives nearly 5¼ degrees for each day's motion: the brightest part or spot is represented as centrally facing us, and accordingly shewing the star in its greatest lustre. Were this bright spot and the other parts to remain *unchangeable* they would after having completed the revolution of 360 degrees or 62 days, (the star's rotation on its axis,) appear again as at first, and at every return continue to give exact periodical times, as was nearly the case in 1799 between August and October, (See Table IX. p. 142,) but if the spot becomes

obscure and another brightens up in a different place; this latter will make the star appear at its next full splendour either sooner or later than the real rotation according to its position, thus,

*2d View.*

Fig. 1. A full brightness having been shewn by the same spot, it afterwards loses its light and another as bright is produced 5 days motion (or 29 degrees) preceding it at E, see Fig. 2. This latter, when turned centrally to the earth, will appear *5 days sooner* than the former one, now obscured, (here marked P,) and show the star at its full lustre, making the rotation 57 days instead of 62, which was the case in 1796, the observed revolution between September 17 and November 13. (See Table IX.)

*3d View.*

Fig. 3. We will now apply a case of an interval of too great length, that of 72 days: the spot *m* alone having shewn us the star in its full lustre, its light disappears during the revolution, and another brightens forth ten days (or 58 degrees) *following it* at H; when *m* returns to face us again in 62 days it being obliterated, the star will still appear obscured, and not recover its splendour until the new brightened part H becomes central, which being *ten days later* than the position in which *m* was seen, makes the revolution 72 days instead of 62, as was observed between July 14 and September 24, 1801. (See Table IX.) In the above case the alterations took place while behind the star, otherwise some irregularities would have been perceived, as will later be noticed. The same reasoning with proper alterations will, I apprehend, account for the other revolutions, yet I shall soon again resume the subject with

regard to a *series* of the greatest irregularities; at present let us proceed to take a few views of the intervals of its *least brightness*, which, contrary to my expectation, I find much more difficult to explain than those of the full, although the results disagree less among themselves. The darkened face of the star is here represented with a few small changeable bright spots, placed in general, at a proper distance, so as to keep up an uninterrupted increase and decrease of light with regard to us, and are also made to correspond with several other observations.

*4th View.*

Fig. 4 is to explain the greatest interval of 74 days, between July 4th, and September 16th 1799. (See Table X.) The darkened hemisphere here exhibited is its *minimum* July 4th, with the following spots, *w* nearly gone off, next a small one *l*, then another P of a similar size, preceding the centre a day or two, (or a few degrees,) and lastly a bright one at D, just appearing. During the rotation, D losing its light and the P becoming *much brighter*, the star at its next return in 62 days, when at its first position, must of course appear much brighter, (See fig. 3) but by the retiring of *l* and P continues to diminish in lustre till the appearance of some large spot from the other hemisphere; which taking place 12 days afterwards, will, (when this time is added to the 62 already revolved) make the revolution of 74 days, as required; for a view of a short interval, for the present let that of 56 days be taken, between August 21st and October 16th 1801. (See Table X.)

*5th View.*

The least brightness or *minimum* is represented by fig. 6, when the bright spots *y* and *x* at each extremity of the equatorial diameter are mutually but just in sight and a minute one, *r* alone on its surface preceding *y* by 6 days motion: *n n*, are other middling sized spots near *x*, but preceding it; they cannot for the present be seen, being on the opposite or bright hemisphere. The spot *x* during the stars revolution having lost its light, and *r* being considerably increased, the next *minimum* will be between *n n* and *r*, (instead of *x* and *y*.) See fig. 7; and by the retiring of *n n* the *diminution* of the star's light will continue to take place only until the reappearance of *r*, at the place where *y* was, which being 6 days sooner than the former position, (See fig. 6,) reduces the rotation to 56 days. All the foregoing views are from unconnected periods, where only the ultimate returns of each appearance have been attended to; but now, I shall examine a long interval with many intermediate changes, that between June 18th, and September 17th 1796, wherein are included the most intricate irregularities and vicissitudes: these observations are already printed at full length in the Philosophical Transactions for 1797, and therefore can at any time be inspected: indeed, I then little thought they would ever become of further use, but that of stating facts, to which, however, I have always been very partial, and particularly so, after having experienced the advantage of MARALDI's printed observations on the variable star in Hydra, as it was partly by them that I ascertained the periodical returns of brightness of that star, and which flattered me the more, as MARALDI himself had been less successful in

the attempt: See Phil. Trans. for 1786. Yet in the present Paper I have omitted all such details, being aware they might be thought too voluminous, but hope at some future time the Society will honour them with a place in their library.

The first sketch, Plate III. represents, for June 13, 1796, the comparative size of the bright spots supposed to surround the star, but here extended at full length; the next eight following are spherical views, on an enlarged scale, for each quarterly rotation or less, shewing the principal changes, as expressed in the adjoining remarks, and corresponding with the observations; these being taken from my printed paper, as already mentioned, are marked in italics. It will be seen that the spots by which the changes are principally regulated, are placed at equal distances, yet intermediate ones might also frequently be inserted without occasioning any objection, but that of rendering the explanations more complex.

## REMARKS ON PLATE III.

Fig. 2. "*June 18th. Full brightness Mag. bright 5th,*" before or after which date the star would appear less bright, by the spot E being removed from the centre, and one of the others out of view.

Fig. 3. "*July 3d, 15 days or ½ rotation being elapsed since June 18th, 5th Mag. a little decreased*" by the removal of the brightest spot E, the *h* being much less.

Fig. 4. "*July 19th, 16 days or ½ rotation 5.6 Mag. still decreased,*" N being much less than *h*, now gone off. "*A slight minimum.*"

Fig. 5. "*July 27th, 8 days of the rotation, 5 Mag. rather*

*increased*" by the considerable increase of N since four days, with the addition of F, *a slight full brightness*.

Fig. 6. "*Aug. 3d, 7 days of rotation, 5.6 Mag. decreased* by the going off of N, the E, which is now reappearing, being reduced to much less than F.

Fig. 7. "*Aug. 19th, 16 days or ½ rotation, 5.6 Mag. again decreased*," by the removal of F, by E being much less, and by the *h* also being considerably diminished.

Fig. 8. "*Sept. 3d, 15 days or ½ rotation, 6 Mag. still more decreased*," by the *h* being much less than E, which is now going off, and N scarcely reappearing, *another minimum*.

Fig. 9. "*Sept. 17th, 14 days or near ½ rotation. 5 Mag. full brightness considerably increased*," by N having retained its increased brightness of July 27, and now facing us centrally.

1st, Thus are exhibited, the two short intervals of its full brightness, one between June 18 and July 27, of 39 days, and the other between July 27 and Sept. 17, of 52 days. See Table IX.

2dly, The interval of 46 days between the two *minima* of July 19 and Sept. 3; See Table X.

3dly, The long decrease of 38 days between July 27 and Sept. 3, and

4thly, The rapid increases of 3 and 14 days between the 19 and 27th of July, and the 3d and 17th of September,

As also the other intermediate changes, yet I must again repeat, particularly as a few days error may occasionally proceed from the observations, that by these sketches it is not meant to give exact drawings of the size, distances or alterations of the spots, but merely to shew how the changes may take place, as, I believe, nothing of the kind has hitherto been

offered to the public, either with or without corroborating observations; nor do I presume to think, that the explanations are the only ones or best that can be imagined, the more so, as they solely refer (for greater simplicity) to the star's equator, while possibly, were the spots placed in a northern or southern latitude, or permanent ones near the poles, or were a proper inclination, given to the polar axis, they might be more satisfactory: however, the materials themselves, the *observations* and *deductions* will I flatter myself ever be acceptable, and contribute to facilitate future conjectures, which from an allowable analogy may extend to similar parts of the starry system, with regard to the probability of establishing whether any of the most *irregular or particular* changes may not *return at first periods*, or after a certain number of rotations. I think we can entertain but slight hopes of it, owing to the *great fluctuation* of the luminous matter, as shewn by the *perpetual varying* of the *apparent* revolutions, magnitudes &c. See Tab. IX. X. and VII. Still it is natural to suppose, that some parts of the atmosphere of this star may have a less tendency than others to become luminous, so as to promote at different times, similar appearances; and indeed this is strongly indicated by the *intervals* of the *minima* being far *more regular* than those of the *full brightness*, which, with other reasons induce us to suspect that even one of its hemispheres is less favourably constituted or qualified, than the other for the generating of these particles, although they do occasionally encroach on both sides, as appears by the observations between June and August, See Phil. Trans. for 1797, or the eight sketches of 1796, and likewise in 1797, see Tab. VII. when during *three months* it was only reduced to the 5 or 6 Mag. by which the degree of brightness that surrounded

it, must have been nearly equal: had the causes of varying its light then ceased, it would ever have continued to appear as an unchangeable star of the 5 or 6 Mag. and such is the case of several others that *formerly have been variables*, but for many years retain a steady brightness, as ϑ Geminorum, ζ Ursæ majoris, α Draconis, and perhaps that in the Swan's breast, while others, after *shewing their changes*, have *entirely disappeared*, owing to a total absorption of light, as the famous one in Cassiopea, in Serpentarius of 1604, that near the Swan's head, and doubtless many more. Does not this induce us to presume that there are also others, that have *never shewn* a glimpse of brightness? Lastly, *new variables* may become so at different periods, by an unusual and partial increase or diminution of their bright parts, as not unlikely was the case of ο Ceti, Algol α Herculis, &c. for these stars being by times very conspicuous, their changes, had they been always equally great, might have been easily noticed by the ancient astronomers, who observed only with the *naked eye*. A few lines above, I mentioned the probability that there existed *primary* invisible bodies or *unenlightened stars* (if I may be allowed the expression) that have ever remained in *eternal darkness*; how numerous these may be, can never be known. Would it then be too daring or visionary to suppose their numbers equal to those endowed with light? particularly when we take into contemplation the ample set of bodies visible only by reflected rays, that appertain to our own system, such as the planets, asteroides, comets, and satellites. Do not these, although but of a secondary nature, lead us to venture on the foregoing more enlarged conjecture; and moreover to suspect, that the *enlightened stars* are those that have already attained the highest degree of perfection? granting, therefore, such

multitudes do really exist, clusters of them, by being collected together as in the milky-way, must intercept all more distant rays; and if this from any intervening lights, they would appear as *dark spaces* in the heavens, similar to what has been observed in the Southern Hemisphere. That so few of these objects should be perceived, may be attributed to their being obliterated by the presence either of some scattered stars, or of other slight luminous appearances.

I have thus fully investigated the nature of this distant sun, a single one among many millions, and scarcely perceptible to the sight, yet of no less importance than our own grand luminary. But ours is still supplied abundantly with resplendent particles, while Sobieski's variable star has them most sparingly dispersed over its sphere: a scantiness that apparently must occasion to its surrounding planets, constant vicissitudes of uncertain darkness, and repletion of light and heat. How far more enviable seems our situation! I mean that which we enjoy at present; there being strong reasons to believe, that the sun's luminous appearance has been at times considerably diminished; and I have little hesitation in conceiving that it may also be reduced at some future period to small patches, and then the apparent irregularities of its periodical rotations, which at present are only perceived by the observations of trifling dark spots, would become evidently conspicuous, particularly when seen at a distance as remote as the variable stars are from us. But such conjectural flights of fancy cannot too soon be dropt. I therefore shall conclude with observing, that these inquiries on the alterations of light of the stars have been so little discussed, that it is to be hoped they will not be discontinued; and although I have already

troubled the Society with many papers concerning such changes, I need not here enlarge any longer, having the honour of presenting them with one more, most probably my last, on this subject.

EDW. PIGOTT.

Fig. 1.

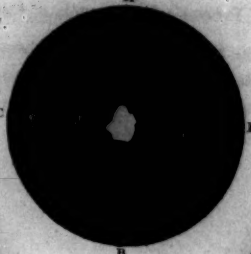



Fig. 2.

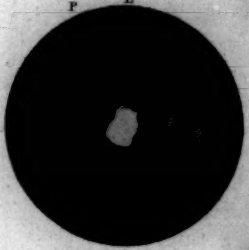



Fig. 3.

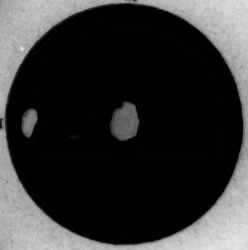



Fig. 4.

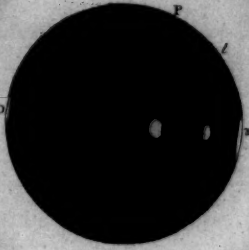



Fig. 5.

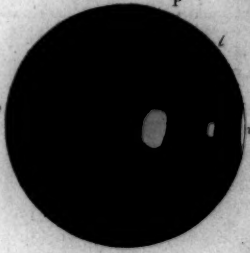



Fig. 6.

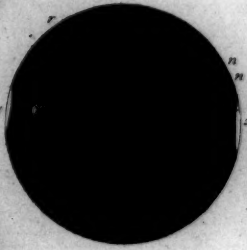
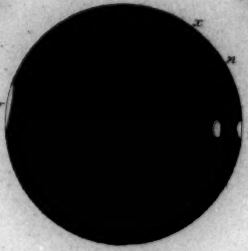
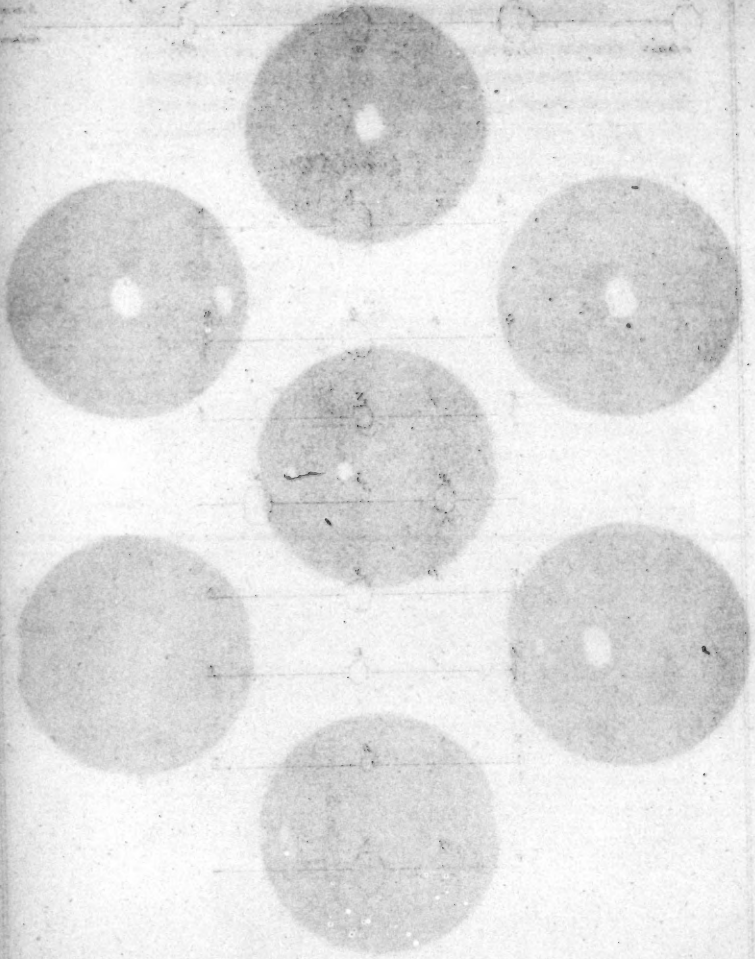



Fig. 7.

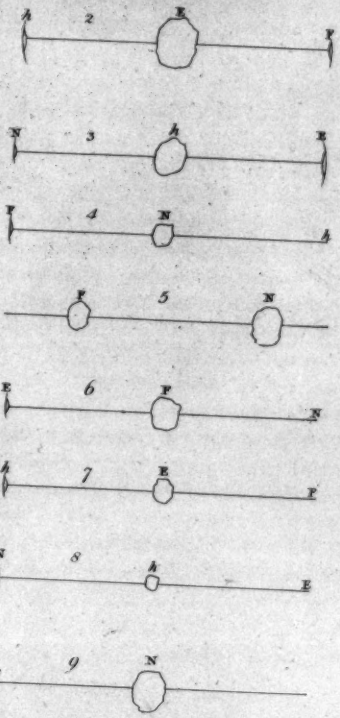



Basire sc.



## An extended View of the Surrounding Spots June 18. 1796

## Spherical Views




Basire sc

## VII. *An Account of some analytical Experiments on a mineral Production from Devonshire, consisting principally of Alumine and Water.* By Humphry Davy, *Esq. F. R. S. Professor of Chemistry in the Royal Institution.*

Read February 28, 1805.

### I. *Preliminary Observations.*

This fossil was found many years ago by Dr. Wavel, in a quarry near Barnstaple: Mr. Hatchett, who visited the place in 1796, described it as filling some of the cavities and veins in a rock of soft argillaceous shist. When first made known, it was considered as a zeolite; Mr. Hatchett, however, concluded, from its geological position, that it most probably did not belong to that class of stones; and Dr. Babington, from its physical characters, and from some experiments on its solution in acids, made at his request by Mr. Stockler, ascertained that it was a mineral body, as yet not described, and that it contained a considerable proportion of aluminous earth.

It is to Dr. Babington that I am obliged for the opportunity of making a general investigation of its chemical nature; and that gentleman liberally supplied me with specimens for analysis.

### II. *Sensible Characters of the Fossil.*

The most common appearance of the fossil is in small hemispherical groups of crystals, composed of a number of filaments radiating from a common centre, and inserted on the surface of the shist; but in some instances it exists as a collection of irregularly disposed prisms forming small veins in the stone: as yet, I believe, no insulated or distinct crystal has been found. Its colour is white, in a few cases with a tinge of gray or of green, and in some pieces (apparently beginning to decompose) of yellow. Its lustre is silky; some of the specimens possess semi-transparency, but in general it is nearly opaque. Its texture is loose, but its small fragments possess great hardness, so as to scratch agate.

It produces no effect on the smell when breathed upon, has no taste, does not become electrical or phosphorescent by heat or friction, and does not adhere to the tongue till after it has been strongly ignited. It does not decrepitate before the flame of the blow-pipe; but it loses its hardness, and becomes quite opaque. In consequence of the minuteness of the portions in which it is found, few of them exceeding the size of a pea, it is very difficult to ascertain its specific gravity with any precision; but from several trials I am disposed to believe, that it does not exceed 2,70, that of water being considered as 1,00.

### III. *Chemical Characters of the Fossil.*

The perfectly white and semi-transparent specimens of the fossil are soluble both in the mineral acids and in fixed alkaline lixivia by heat, without sensibly effervescing and without

leaving any notable residuum; but a small part remains undissolved, when coloured or opaque specimens are exposed to the alkaline lixivia.

A small semi-transparent piece, acted on by the highest heat of an excellent forge, had its crystalline texture destroyed, and was rendered opaque; but it did not enter into fusion. After the experiment it adhered strongly to the tongue, and was found to have lost more than a fourth of its weight. Water and alcohol, whether hot or cold, had no effect on the fossil. When it was acted on by a heat of from 212 to 600° Fahrenheit in a glass tube, it gave out an elastic vapour, which when condensed appeared as a clear fluid possessing a slight empyreumatic smell, but no taste different from that of pure water.

The solution of the fossil in sulphuric acid, when evaporated sufficiently, deposited crystals which appeared in thin plates, and had all the properties of sulphate of alumine; and the solid matter, when redissolved and mixed with a little carbonate of potash, slowly deposited octahedral crystals of alum. The solid matter precipitated from the solution of the white and semi-transparent fossil in muriatic acid, was in no manner acted upon by solution of carbonate of ammonia, and therefore it could not contain any glucine or ittria; and its perfect solubility without residuum in alkaline lixivia shewed that it was alumine.

When the opaque *varieties* of the fossil were fully exposed to the agency of alkaline lixivia, the residuum never amounted to more than one-twentieth part of the weight of the *whole*. In the white opaque variety, it was merely calcareous earth, for when dissolved in muriatic acid, not in excess,

it gave a white precipitate when mixed with solution of oxalate of ammonia, and did not affect solution of prussiate of potash and iron.

In the green opaque variety, calcareous earth was indicated by solution of oxalate of ammonia: and it contained oxide of manganese; for it was not precipitated by solution of ammonia; but was rendered turbid, and of a gray colour, by solution of prussiate of potash and iron.

The residuum of the alkaline solution of the yellow variety, when dissolved in muriatic acid, produced a small quantity of white solid matter when mixed with the solution of the oxalate of ammonia, and gave a light yellow precipitate by exposure to ammonia; but after this, when neutralized, it did not affect prussiate of potash and iron, so that its colouring matter, as there is every reason to believe, was *oxide of iron*.

IV. *Analysis of the Fossil.*

Eighty grains of the fossil consisting of the whitest and most transparent parts that could be obtained, were introduced into a small glass tube having a bulb of sufficient capacity to receive them with great ease. To the end of this tube, a small glass globe attached to another tube, communicating with a pneumatic mercurial apparatus, was joined by fusion by means of the blow-pipe.

The bulb of the tube was exposed to the heat of an Argand lamp; and the globe was preserved cool by being placed in a vessel of cold water. In consequence of this arrangement, the fluid disengaged by the heat, became condensed, and no elastic matter could be lost. The process was continued for half an hour, when the glass tube was quite red.

A very minute portion only of permanently elastic fluid passed into the pneumatic apparatus, and when examined, it proved to be common air. The quantity of clear fluid collected, when poured into another vessel, weighed 19 grains, but when the interior of the apparatus had been carefully wiped and dried, the whole loss indicated was 21 grains. The 19 grains of fluid had a faint smell, similar to that of burning peat; it was transparent, and tasted like distilled water; but it slightly reddened litmus paper. It produced no cloudiness in solutions of muriate of barytes, of acetite of lead, of nitrate of silver, or of sulphate of iron.

The 59 grains of solid matter were dissolved in diluted sulphuric acid, which left no residuum; and the solution was mixed with potash in sufficient quantity to cause the alumine at first precipitated again to dissolve. What remained undissolved by potash, after being collected and properly washed, was heated strongly and weighed: its quantity was a grain and quarter. It was white, caustic to the taste, and had all the properties of lime.

The solution was mixed with nitric acid till it *became sour*. Solution of carbonate of ammonia was then poured into it till the effect of decomposition ceased. The whole thrown into a filtrating apparatus left solid matter, which when carefully washed and dried at the heat of ignition, weighed 56 grains. They were pure alumine: hence the general results of the experiments, when calculated upon, indicated for 100 parts of this specimen,

| | |
|---|---|
| Of alumine - - - - | 70 |
| Of lime - - - - | 1.4 |
| Of fluid - - - - | 26.2 |
| Loss - - - - | 2.4 |

The loss I am inclined to attribute to some fluid remaining in the stone after the process of distillation; for I have found, from several experiments, that a red heat is not sufficient to expel all the matter capable of being volatilized, and that the full effect can only be produced by a strong white heat.

Fifty grains of a very transparent part of the fossil, by being exposed in a red heat for fifteen minutes, lost 13 grains; but when they were heated to whiteness, the deficiency amounted to 15 grains, and the case was similar in other trials.

Different specimens of the fossil were examined with great care, for the purpose of ascertaining whether any minute portion of fixed alkali existed in them; but no indications of this substance could be observed; the processes were conducted by means of solution of the unaltered fossil in nitric acid; the earths and oxides were precipitated from the solution by being boiled with carbonate of ammonia; and after their separation, the fluid was evaporated to dryness, and the nitrate of ammonia decomposed by heat, when no residuum occurred.

A comparative analysis of 50 grains of a very pellucid specimen was made by solution in lixivium of potash. This specimen lost 8 grains by long continued ignition, after which it easily dissolved in the lixivium by heat, leaving a residuum of a quarter of a grain only, which was red oxide of iron. The precipitate from the solution of potash, made by means of muriate of ammonia, weighed, when properly treated, 21 grains.

Several specimens were distilled in the manner above described, and in all cases the water collected had similar properties. The only test by which the presence of acid matter in it could be detected, was litmus paper; and in some cases the effect upon this substance was barely perceptible.

## V. General Observation.

I have made several experiments with the hope of ascertaining the nature of the acid matter in the water; but from the impossibility of procuring any considerable quantity of the fossil, they have been wholly unsuccessful. It is, however, evident, from the experiments already detailed, that it is not one of the known mineral acids.

I am disposed to believe, from the minuteness of its proportion, and from the difference of this proportion in different cases, that it is not essential to the composition of the stone; and that, as well as the oxide of manganese, that of iron, and the lime it is only an accidental ingredient, and on this idea the pure matter of the fossil must be considered as a chemical combination *of about thirty parts of water and seventy of alumine.*

The experiments of M. THEODORE DE SAUSSURE on the precipitation* of alumine from its solutions, have demonstrated the affinity of this body for water; but as yet I believe no aluminous stone, except that which I have just described, has been found, containing so large a proportion of water, as thirty parts in the hundred.

The diaspore, which has been examined by M. VAUQUELIN, and which loses sixteen or seventeen parts in the hundred by ignition, and which contains nearly eighty of alumine, and only three of oxide of iron, is supposed by that excellent chemist to be a compound of alumine and water. Its physical and chemical characters differ however very much from those of the new fossil, and other researches are wanting to ascertain whether the part of it volatilized by heat is of the same kind.

* Journal de Physique, Tom. LII. p. 280.

I have examined a fossil from near St. Austle, in Cornwall, very similar to the fossil from Barnstaple in all *its general chemical characters*; and I have been informed, that an analysis of it, made by the Rev. WILLIAM GREGOR some months since, proves that it consists of similar ingredients.

Dr. BABINGTON has proposed to call the fossil from Devonshire *Wavellite*, from Dr. WAVEL, the gentleman who discovered it; but if a name founded upon its chemical composition be preferred, it may be denominated *Hydrargillite*, from ὕδωρ water, and ἄργιλλος clay.

## VIII. *Experiments on Wootz. By Mr. David Mushet. Communicated by the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read February 14, 1805.

The following experiments were made at the request of Sir Joseph Banks, on five cakes of wootz, with which he supplied me for that purpose. As the cakes, which were numbered 1, 2, 3, 4, 5, were not all of the same quality, it will be proper first to describe the differences observable in their external form and appearance.

No. 1 was a dense solid cake, without any flaw or fungous appearance upon the flat, or, what I suppose to be, the upper side. The round or under surface was covered with small pits or hollows, two of which were of considerable depth; one through which the slit or cut had run, and another nearly as large towards the edge of the cake. These depressions, the effects, as I suppose, of a species of crystallization in cooling, were continued round the edges, and even approached a little way upon the upper surface of the wootz.

The cake was a quarter of an inch thicker at one extremity of the diameter than at the other, from which I infer, that the pot or crucible, in which this cake had been made, had not occupied the furnace in a vertical position. Its convexity, compared to that of the other five, was second. Upon breaking the thin fin of steel, which connects the half cakes together, I found it to possess a very small dense white grain. This appearance never takes

place but with steel of the best quality, and is less frequent in very high steel, though the quality be otherwise good.

Upon examining the break with attention, I perceived several laminæ and minute cells filled with rust, which in working are never expected to unite or shut together. The grain otherwise was uniformly regular in point of colour and size, and possessed a favourable appearance of steel.

No. 2. This cake had two very different aspects; one side was dense and regular, the other hollow, spongy, and protuberant. The under surface was more uniformly honey-combed than No. 1; the convexity in the middle was greater, but towards the edges, particularly on one side, it became flatter. The grain exposed by breaking was larger, paler in colour, and more sparkling than No. 1. In breaking, the fracture took but slightly out, and displayed the same unconnected laminæ with rusty surfaces, as were observed in No. 1. Beside these, two thin fins of malleable iron projected from the unsound side, and seemed incorporated with the mass of steel throughout. Towards the centre of the break, and near to the excrescence common to all the cakes, groups of malleable grains were distinctly visible. The same appearance, though in a slighter degree, manifested itself in various places throughout the break.

No. 3. The upper surface of this cake contained several deep pits, which seemed to result from the want of proper fluidity in fusion. They differed materially from those described upon the convex sides of No. 1 and 2, and were of that kind that would materially effect the steel in forging.

The under or convex side of this cake presented a few crystalline depressions, and those very small: the convexity

was greater than that of No. 1 and 2, the fracture of the fin almost smooth, and only in one place exhibited a small degree of tenacity in the act of parting. In the middle of the break, about half an inch of soft steel was evident; and in different spots throughout numerous groups of malleable grains, and thin laminæ of soft blue tough iron made their appearance.

No. 4. Was a thick dense cake possessed of the greatest convexity; the depressions upon the under side were neither so large, nor so numerous as those in No. 1 and 2, nor did they approach the upper surface of the cake further than the acute edge. This surface had the most evident marks of hammering to depress the feeder, or fungous part of the metal, which in the manufacturing seems the gate or orifice by which the metal descends in the act of gravitation.

The break of this cake, however favourable as to external appearance, was far from being solid. Towards the feeder it seemed loose and crumbly, and much oxidated. The grain divided itself into two distinct strata, one of a dense whitish colour, the other large and bluish, containing a number of small specks of great brilliancy. Several irregular lines of malleable iron pervaded the mass in various places, which indicated a compound too heterogeneous for good steel.

No. 5th cake. This was materially different in appearance from any of the former. It had received but little hammering, yet was smooth and free from depressions, or honey-comb on both surfaces. The feeder, instead of being an excrescence, presented a deep concave beautifully crystallized.

In breaking, the fracture tore out considerably, but presented a very irregular quality of grain. That towards the under surface was small and uniform, but towards the flat or

upper surface it increased in size, and in the blueness of its colour, till it passed into the state of malleable iron.

The break of this steel, though apparently soft, was the least homogeneous of the whole, and throughout it presented a very brilliant arrangement of crystal, which in other steel is always viewed with suspicion.

*General Remark.*

Uniformly the grain and density of the wootz are homogeneous, and free from malleable iron towards the under or round surface; but always the reverse towards the feeder or upper side.

*Remarks in Forging.*

No. 1. One-half of the cake was heated slowly by an annealing heat to a deep red, and put under a sharp broad-mouthed chissel with a small degree of taper. It cut with difficulty, was reheated, and cracked a little towards one end of the slit or cut originally in the cake.

The heat in this trial was so moderate, that I was afraid that the crack had arisen from a want of tenacity, occasioned by the heat being too low.

The other half was heated a few shades higher, and subjected to the same mode of cutting; before the chissel had half way reached the bottom, the piece parted in two in the direction of the depression made by the cutting instrument. The additional heat in this instance proved an injury, while the cracking of the steel in both cases, particularly the former, was a certain proof of the abundance, or rather of the excess of the steely principle.

The fractures of both half cakes, now obtained for a second time, were materially different from that obtained by the simple division of the cake. The grain was nearly uniform, distinctly marked, but of too gray a colour for serviceable steel. Two of the quarters being drawn into neat bars under hand hammers at a low heat, one of them contained a number of cracks and fissures. The fracture was gray, tore out a little in breaking, but was otherwise yolky and excessively dense. A small bar of penknife size was improved greatly in drawing down, and had only one crack in thirteen inches of length. The grain and fracture were both highly improved by this additional labour; the tenacity of the steel was greater, and it stood firmly under the hammer at a bright red heat.

The other two quarters of this cake were squared a little, and successively put under a tilt hammer, of two hundred weight, going at the rate of three hundred blows per minute, and drawn into small penknife size. One of the bars from an outside piece, always the most solid, was entirely free from cracks, and had only one small scale running upon one side.

These bars exhibited a tougher break, than those drawn by hand; the colour was whiter, and the grain possessed a more regular and silky appearance.

*Forging* No. 2.

One half of this cake was heated to a scarlet shade, and put under the cutting chissel; it was at first struck lightly, then reheated, and cut comparatively soft; but a small crack had over-run the progress of the chissel. Its softness in cutting was attributed to an evident want of solidity. The other half cake felt harder under the hammer, but proved afterwards

spongy throughout the mass. In the act of cutting, a loose pulverized matter was disengaged from some of the cells, possessed of a shining appearance.

The fractures obtained in consequence of the division of the half cakes, presented a flattish crystallized appearance, more resembling very white cast iron, than steel capable of being extended under the hammer. One of the middle cuts was entirely cellular with crystallized interiors, and incapable of drawing; the corresponding cut of the other half cake was drawn into a straight bar three quarters of an inch in breadth, and three-eighths thick, but was covered with cracks and flaws from end to end. The colour of the break was one shade lighter than No. 1, it tore less out, was equally yolky, and possessed on the whole an aspect very unfavourable for good steel.

The other two outside quarters were also drawn into shape, one under the tilt hammer, and the other by hand. These were more solid in the fracture, possessed fewer surface-cracks, stood a higher degree of heat, tore out more, and exhibited a silky glossy grain, at least two shades lighter in the colour than the centre pieces.

*Forging 3d Cake.*

One half of this cake, first subjected to be cut, was found softer than any of the preceding, and exhibited no symptom of cracking. The other half was cut at three heats, but found loose and hollow in the extreme. A considerable portion of the same brilliant powder, formerly noticed, was here again disengaged. It was carefully taken up for examination, and found to be very fine ore of iron in a pulveresent

state, very obedient to the magnet, and without any doubt an unmetallized portion of that from which wootz is made.

This curious circumstance led me to examine every pore and cell throughout the whole fragments. On the upper surface of two of them I found small pits containing a portion of the ore, which had been slightly agglutinated in the fire, but still highly magnetic. The upper surface of the present cake, close by the gate or feeder, contained a large pit filled with a stratum of semi-fused ore, surmounted by a mass of vitrified matter, which bore evident marks of containing calcareous earth.

Those who have devoted sufficient attention to the affinities of iron and earths for carbon, will be surprised to find that, on this particular subject, the rude fabricators of steel in Hindostan have got the start of our more polished countrymen in the manufacture of steel.

Two bars of wootz were formed from this cake, and these in point of quality inferior to any of those formerly produced. The appearance of the metal was more varied, less homogeneous, and contained more distinct laminæ with rusty surfaces, than either of the two former cakes.

It appeared highly probable, from the observations that occurred in forging, and in the examination of the cake, that the original proportion of mixture was such as would have formed a quality of steel softer than No. 1 and 2; but as steel of such softness requires a greater heat to fuse it, than when more fully saturated with carbonaceous matter, it is probable that the furnace had not been sufficiently powerful to occasion complete fusion of the whole mass, and generate a steel homogeneous in all its parts.

*Forging 4th Cake.*

Both halves of this cake cut pleasantly, and with a degree of tenacity and resistance, mixed at the same time with softness beyond what was experienced in any of the former cakes. Two quarters of this cake were drawn under the tilt hammer, and one by hand. The resulting bars were nearly perfect. A slight scale was observable upon the bar, from that quarter which contained the figure. The fracture was solid, though not homogeneous as to quality and colour, and it appeared pretty evident, that a considerable portion of one side through the whole bar was in the state of malleable iron, and of course not capable of being hardened. It was a subject of considerable regret, that the cake the most perfect and the most tenacious of the whole, in the process of forging, should get an imperfection which rendered it useless for the perfect purposes of steel.

*Forging 5th Cake.*

The first half of this cake cut uncommonly soft for wootz, but by cracking before the chissel still exhibited a want of proper tenacity. The next half cut equally soft, but with more tenacity. Two quarters of this cake drew readily out under the tilt hammer, and a third was drawn by hand at a bright red, sometimes approaching to a faint white heat. None of the bars thus obtained were uniformly free from cracks and scale, although the fracture exhibited a fair break of a light blue colour, and the grain was distinctly marked, and free from yolks.

*General Remarks.*

The formation of wootz appears to me to be in consequence of the fusion of a peculiar ore, perhaps calcareous, or rendered highly so by mixture of calcareous earth along with a portion of carbonaceous matter. That this is performed in a clay or other vessel or crucible, is equally presumable, in which the separated metal is allowed to cool; hence the crystallization that occupies the pits and cells found in and upon the under or rounded surface of the wootz cakes.

The want of homogeneity, and of real solidity in almost every cake of wootz, appears to me to be a direct consequence of the want of heat sufficiently powerful to effect a perfect reduction; what strengthens this supposition much is, that those cakes that are the hardest, *i. e.* that contain the greatest quantity of carbonaceous matter, and of course form the most fusible steel, are always the most solid and homogeneous. On the contrary, those cakes, into which the cutting chissel most easily finds its way, are in general cellular, replete with laminæ, and abound in veins of malleable iron.

It is probable, had the native Hindostan the means of rendering his cast steel as fluid as water, it would have occurred to him to have run it into moulds, and by this means have acquired an article uniform in its quality, and convenient for those purposes to which it is applied.

The hammering, which is evident around the feeder and upon the upper surface in general, may thus be accounted for. When the cake is taken from the pot or crucible, the feeder will most probably be slightly elevated, and the top of the cake partially covered with small masses of ore and steel iron,

which the paucity of the heat had left either imperfectly separated or unfused. These most probably, to make the product more marketable, are cut off at a second heating, and the whole surface hammered smooth.

I have observed the same facts and similar appearances in operations of a like nature, and can account satisfactorily for it as follows.

The first portions of metal, that are separated in experiments of this nature, contain the largest share of the whole carbon introduced into the mixture. It follows of course, that an inferior degree of heat will maintain this portion of metal in a state of fluidity, but that a much higher temperature is requisite to reduce the particles of metal, thus for a season robbed of their carbon, and bring them into contact with the portion first rendered fluid, to receive their proportion of the steely principle. Where the heat is languid, the descent of the last portions of iron is sluggish, the mass below begins to lose its fluidity, while its disposition for giving out carbon is reduced by the gradual addition of more iron. An accumulation takes place of metallic masses of various diameters, rising up for half an inch or more into the glass that covers the metal; these are neatly welded and inserted into each other, and diminish in diameter as they go up. The length, or even the existence of this feeder or excrescence, depends upon the heat in general, and upon its temperature at different periods of the same process. If there has been sufficient heat, the surface will be convex and uniformly crystalline; but if the heat has been urged, after the feeder has been formed and an affinity established between it and the steelified mass below, it will only partially disappear in the latter, and the head or part of

the upper end of the feeder will be found suspended in the glass that covers the steel.

The same or similar phenomena take place in separating crude iron from its ores, when highly carbonated, and difficult, from an excess of carbon, of being fused.

The division of the wootz cake by the manufacturers of Hindostan, I apprehend is merely to facilitate its subsequent application to the purposes of the artist; it may serve at the same time as a test of the quality of the steel.

To ascertain, by direct experiment, whether wootz owed its hardness to an extra quantity of carbon, the following experiments were performed with various portions of wootz of common cast steel, and of white crude iron, premising that in operations with iron and its ores, I have always found the comparative measure of carbon best ascertained by the quantity of lead which was reduced from flint glass.

*1st Cake.*

| | Grains. |
|---|---|
| Fragments of wootz - - - - | 65 |
| Pounded flint glass three times the weight - | 195 |

This mixture was exposed to a heat of 160° Wedgewood, and the wootz fused into a well crystallized spherule of steel. A thin crust of revived lead was found below the wootz, which weighed 9 grains, or $\frac{112}{1000}$ the weight of the wootz.

*2d Cake.*

| | |
|---|---|
| Fragments of wootz - - - - | 80 |
| Flint glass, same proportion as above - - | 240 |

The fusion of the mixture in this experiment was productive of a mass of lead weighing 10 grains, equal to $\frac{1}{8}$th the weight of the wootz.

*3d Cake.* Grains.

Fragments of wootz - - - - 75

Flint glass - - - - 225

The mass of lead precipitated beneath the steel in this experiment, amounted to 9 grains, or $\frac{13}{100}$ the weight of the wootz employed.

*4th Cake.*

Fragments of wootz - - - - 93

Flint glass - - - - 279

Lead obtained, precipitated from the glass by means of the carbon of the wootz 14½ grains, equal to $\frac{144}{1000}$ the weight of the wootz.

*5th Cake.*

Fragments of wootz - - - - 69

Flint glass - - - - 207

The lead revived in this experiment amounted to 7 grains, which is equal to $\frac{103}{1000}$ the weight of the wootz.

*6th. Cast Steel formed with $\frac{1}{60}$th part its Weight of Carbon.*

Fragments - - - - - 90

Crystal glass - - - - 270

Lead revived 8½ grains equal to $\frac{94}{1000}$ the weight of the steel introduced.

*7th. White cast Iron dropt while Fluid into Water.*

Fragments - - - - - 103

Crystal glass - - - - 309

The fusion of this precipitated 29½ grains of lead which is equal to $\frac{333}{1000}$ the weight of the cast iron.

*Recapitulation of these Experiments.*

| | | |
|---|---|---|
| 1st cake of wootz revived of lead | - - | ,139 |
| 2d ditto | - - - - - | ,125 |
| 3d ditto | - - - - - | ,120 |
| 4th ditto | - - - - - | ,156 |
| 5th ditto | - - - - - | ,102 |
| Steel containing $\frac{1}{60}$ of its weight of carbon | - | ,094 |
| Cast iron | - - - - - | ,228 |

It would appear to result from these experiments, that wootz contains a greater proportion of carbonaceous matter, than the common qualities of cast steel in this country, and that some particular cakes approach considerably to the nature of cast iron. This circumstance, added to the imperfect fusion which generally occurs in the formation of wootz, appear to me to be quite sufficient to account for its refractory nature, and unhomogeneous texture.

Notwithstanding the many imperfections with which wootz is loaded, it certainly possesses the radical principles of good steel, and impresses us with a high opinion of the ore from which it is formed.

The possession of this ore for the fabrication of steel and bar iron, might to this country be an object of the highest importance. At present it is a subject of regret, that such a source of wealth cannot be annexed to its capital and talent. Were such an event practicable, then our East India Company might, in their own dominions, supply their stores with a valuable article, and at a much inferior price to any they send from this country.

Results of these Experiments.

| | |
|---|---|
| 1st cake of wootz received of Iead | 1.804 |
| 2d ditto - - - - - | 1.845 |
| 3d ditto - - - - - | 1.80 |
| 4th ditto - - - - - | 1.805 |
| 5th ditto - - - - - | 1.85 |
| Steel containing $\frac{1}{50}$ of it's weight of carbon - | 2.031 |
| Cast iron - - - - - | 2.58 |

It would appear to result from these experiments, that wootz contains a greater proportion of carbonaceous matter, than the common qualities of cast steel in this country, and that some particular cakes approach considerably to the nature of cast iron. This circumstance, added to the imperfect fusion which generally occurs in the formation of wootz, appear to me to be quite sufficient to account for its refractory nature, and unmalleableness texture.

Notwithstanding the many imperfect ones with which wootz is loaded, it certainly possesses the radical principles of good steel, and impresses us with a high opinion of the ore from which it is formed.

The possession of this ore for the fabrication of steel and bar iron, might to this country be an object of the highest importance. At present it is a subject of regret, that such a source of wealth cannot be annexed to its capital and talent. Were such an event practicable, then our East India Company might, in their own dominions, supply their stores with a valuable article, and at a much inferior price to any they send from this country.

# METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

# ROYAL SOCIETY,

BY ORDER OF THE

# PRESIDENT AND COUNCIL.

## METEOROLOGICAL JOURNAL
### for January, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | M. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. Points. | Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | ° | ° | Inches. | ° | Inches. | | | |
| Jan. 1 | 37 | 8 | 0 | 37 | 56 | 29,73 | 81 | 0,132 | NNE | 2 | Cloudy. |
| | 43 | 2 | 0 | 43 | 56 | 29,82 | 86 | | NNE | 1 | Cloudy. |
| 2 | 36 | 8 | 0 | 39 | 53 | 29,82 | 91 | | NE | 2 | Cloudy. |
| | 42 | 2 | 0 | 42 | 56 | 29,92 | 83 | | NE | 2 | Fine. |
| 3 | 32 | 8 | 0 | 32 | 52 | 30,13 | 88 | | NE | 1 | Fair. |
| | 40 | 2 | 0 | 40 | 53 | 30,15 | 83 | | NE | 1 | Fair. |
| 4 | 30 | 8 | 0 | 31 | 50 | 30,10 | 83 | | E | 1 | Fair. |
| | 35 | 2 | 0 | 35 | 52 | 30,12 | 83 | | ENE | 1 | Cloudy. |
| 5 | 30 | 8 | 0 | 32 | 49 | 29,85 | 88 | | W | 1 | Snow. |
| | 38 | 2 | 0 | 38 | 51 | 29,72 | 92 | | W | 1 | Cloudy. |
| 6 | 28 | 8 | 0 | 28 | 49 | 29,76 | 85 | | WNW | 1 | Fine. |
| | 34 | 2 | 0 | 34 | 52 | 29,79 | 83 | | NW | 2 | Fair. |
| 7 | 27 | 8 | 0 | 28 | 48 | 29,82 | 89 | | W | 1 | Cloudy. |
| | 35 | 2 | 0 | 35 | 52 | 29,72 | 85 | | WNW | 1 | Cloudy. |
| 8 | 27 | 8 | 0 | 32 | 47 | 29,56 | 85 | | S | 1 | Cloudy. |
| | 36 | 2 | 0 | 36 | 51 | 29,55 | 89 | | SSE | 1 | Cloudy. |
| 9 | 28 | 8 | 0 | 39 | 48 | 29,68 | 91 | | E | 1 | Cloudy. |
| | 42 | 2 | 0 | 42 | 50 | 29,73 | 91 | | SE | 1 | Cloudy. |
| 10 | 40 | 8 | 0 | 38 | 49 | 29,92 | 84 | 0,235 | SE | 2 | Cloudy. [Much wind last night. |
| | 45 | 2 | 0 | 45 | 52 | 29,99 | 80 | | SE | 1 | Fair. |
| 11 | 34 | 8 | 0 | 35 | 48 | 29,92 | 90 | | E | 1 | Cloudy. |
| | 46 | 2 | 0 | 43 | 50 | 29,88 | 89 | | E | 1 | Cloudy. |
| 12 | 43 | 8 | 0 | 45 | 50 | 29,64 | 96 | | SE | 1 | Cloudy. |
| | 49 | 2 | 0 | 49 | 52 | 29,48 | 88 | | SSE | 1 | Cloudy. |
| 13 | 47 | 8 | 0 | 52 | 52 | 29,32 | 96 | 0,023 | S | 2 | Cloudy. |
| | 52 | 2 | 0 | 52 | 54 | 29,22 | 95 | | S | 2 | Rain. |
| 14 | 51 | 8 | 0 | 51 | 53 | 29,25 | 93 | 0,060 | S | 2 | Cloudy. |
| | 54 | 2 | 0 | 54 | 55 | 29,43 | 84 | | S | 2 | Cloudy. |
| 15 | 48 | 8 | 0 | 54 | 53 | 29,56 | 96 | 0,197 | S | 2 | Rain. |
| | 55 | 2 | 0 | 55 | 56 | 29,54 | 97 | | S | 2 | Rain. |
| 16 | 50 | 8 | 0 | 50 | 54 | 29,51 | 96 | 0,075 | E | 1 | Cloudy. |
| | 53 | 2 | 0 | 53 | 57 | 29,45 | 95 | | SSE | 1 | Rain. |

# METEOROLOGICAL JOURNAL
## for January, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|
| | | H. M. | | | Inches. | | Inches. | Points. | Str. | |
| | ° | | ° | ° | | ° | | | | |
| Jan. 17 | 49 | 8 0 | 49 | 55 | 29.41 | 95 | | E | 1 | Rain. |
| | 54 | 2 0 | 54 | 57 | 29.37 | 94 | | SE | 1 | Rain. |
| 18 | 50 | 8 0 | 50 | 57 | 29.58 | 92 | 0,025 | S | 2 | Cloudy. |
| | 51 | 2 0 | 51 | 58 | 29.55 | 92 | | S | 2 | Cloudy. |
| 19 | 47 | 8 0 | 49 | 57 | 29.71 | 94 | | S | 1 | Cloudy. |
| | 53 | 2 0 | 50 | 58 | 29.54 | 92 | | SE | 1 | Cloudy. |
| 20 | 48 | 8 0 | 48 | 56 | 29.19 | 84 | 0,265 | SSW | 2 | Cloudy. [Much wind last night. |
| | 50 | 2 0 | 50 | 57 | 29.30 | 84 | | SSW | 2 | Cloudy. |
| 21 | 45 | 8 0 | 50 | 56 | 29.43 | 94 | 0,055 | S | 2 | Cloudy. |
| | 54 | 2 0 | 54 | 58 | 29.49 | 94 | | S | 2 | Cloudy. |
| 22 | 48 | 8 0 | 48 | 56 | 29.38 | 89 | | S | 2 | Fine. |
| | 53 | 2 0 | 53 | 59 | 29.46 | 78 | | SW | 2 | Fair. |
| 23 | 46 | 8 0 | 46 | 56 | 29.71 | 90 | 0,158 | SW | 1 | Fair. |
| | 52 | 2 0 | 51 | 58 | 29.78 | 84 | | SSW | 2 | Cloudy. |
| 24 | 50 | 8 0 | 50 | 56 | 29.52 | 88 | | SSE | 2 | Cloudy. [Much wind last night. |
| | 53 | 2 0 | 53 | 58 | 29.41 | 83 | | SSE | 2 | Fair. |
| 25 | 48 | 8 0 | 48 | 56 | 29.37 | 90 | | SE | 2 | Cloudy. |
| | 52 | 2 0 | 52 | 58 | 29.34 | 94 | | SE | 2 | Cloudy. |
| 26 | 47 | 8 0 | 48 | 56 | 29.18 | 91 | | SE | 2 | Cloudy. |
| | 52 | 2 0 | 52 | 58 | 29.17 | 83 | | S | 2 | Cloudy. |
| 27 | 47 | 8 0 | 47 | 57 | 29.01 | 92 | | SSE | 1 | Rain. |
| | 51 | 2 0 | 51 | 58 | 29.12 | 88 | | S | 1 | Cloudy. |
| 28 | 47 | 8 0 | 51 | 56 | 28.88 | 92 | | S | 2 | Rain. |
| | 53 | 2 0 | 51 | 57 | 28.68 | 93 | | S | 2 | Rain. |
| 29 | 45 | 8 0 | 45 | 56 | 29.77 | 81 | 0,170 | WNW | 2 | Fair. |
| | 51 | 2 0 | 51 | 58 | 30.05 | 77 | | W | 2 | Fine. |
| 30 | 43 | 8 0 | 47 | 57 | 30.18 | 91 | 0,068 | SSE | 1 | Cloudy. |
| | 51 | 2 0 | 51 | 57 | 30.06 | 90 | | S | 2 | Cloudy. |
| 31 | 48 | 8 0 | 50 | 56 | 29.78 | 90 | 0,210 | SSE | 2 | Cloudy. |
| | 54 | 2 0 | 54 | 58 | 29.68 | 91 | | S | 2 | Cloudy. |

## METEOROLOGICAL JOURNAL

### for February, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | Time. M. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. Points. | Winds. Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | ° | ° | Inches. | ° | Inches. | | | |
| Feb. 1 | 45 | 7 | 0 | 45 | 57 | 29,62 | 90 | | SSW | 1 | Cloudy. |
| | 51 | 2 | 0 | 51 | 59 | 29,55 | 82 | | SSW | 2 | Fair. |
| 2 | 44 | 7 | 0 | 44 | 56 | 29,47 | 89 | 0,160 | S | 1 | Rain. |
| | 50 | 2 | 0 | 50 | 58 | 29,57 | 77 | | W | 1 | Fair. |
| 3 | 45 | 7 | 0 | 45 | 56 | 29,38 | 92 | 0,310 | SW | 1 | Rain. |
| | 49 | 2 | 0 | 49 | 57 | 29,45 | 78 | | WSW | 1 | Cloudy. |
| 4 | 34 | 7 | 0 | 34 | 54 | 29,41 | 83 | 0,022 | WNW | 1 | Cloudy. |
| | 38 | 2 | 0 | 37 | 55 | 29,61 | 87 | | NW | 2 | Fair. |
| 5 | 31 | 7 | 0 | 32 | 52 | 30,05 | 83 | | WNW | 2 | Fair. |
| | 43 | 2 | 0 | 43 | 55 | 29,93 | 77 | | WNW | 2 | Fair. |
| 6 | 27 | 7 | 0 | 27 | 52 | 30,10 | 76 | | NW | 2 | Fine. |
| | 32 | 2 | 0 | 32 | 54 | 30,13 | 71 | | NW | 2 | Fine. |
| 7 | 25 | 7 | 0 | 26 | 49 | 30,34 | 87 | | NNE | 2 | Cloudy. |
| | 35 | 2 | 0 | 35 | 52 | 30,46 | 82 | | NNE | 1 | Fair. |
| 8 | 26 | 7 | 0 | 28 | 48 | 30,49 | 84 | | WNW | 1 | Cloudy. |
| | 40 | 2 | 0 | 39 | 50 | 30,37 | 80 | | W | 1 | Cloudy. |
| 9 | 39 | 7 | 0 | 43 | 49 | 30,05 | 93 | 0,090 | SW | 1 | Cloudy. |
| | 50 | 2 | 0 | 48 | 52 | 29,95 | 92 | | SW | 1 | Rain. |
| 10 | 43 | 7 | 0 | 43 | 51 | 29,68 | 95 | 0,123 | WSW | 1 | Fair. |
| | 51 | 2 | 0 | 51 | 53 | 29,55 | 84 | | S | 2 | Cloudy. |
| 11 | 43 | 7 | 0 | 43 | 51 | 29,05 | 91 | 0,310 | SSW | 2 | Rain. |
| | 50 | 2 | 0 | 48 | 53 | 29,05 | 92 | | W | 1 | Rain. |
| 12 | 42 | 7 | 0 | 42 | 52 | 29,65 | 93 | 0,252 | NE | 1 | Cloudy. |
| | 44 | 2 | 0 | 39 | 52 | 30,02 | 78 | | NE | 1 | Snow. |
| 13 | 31 | 7 | 0 | 31 | 50 | 30,26 | 79 | | NNE | 2 | Fine. |
| | 36 | 2 | 0 | 36 | 53 | 30,32 | 74 | | NE | 2 | Fine. |
| 14 | 27 | 7 | 0 | 28 | 48 | 30,38 | 78 | | NNE | 2 | Fine. |
| | 36 | 2 | 0 | 36 | 50 | 30,37 | 69 | | NE | 2 | Fine. |
| 15 | 27 | 7 | 0 | 28 | 48 | 30,39 | 85 | | NE | 1 | Fine. |
| | 41 | 2 | 0 | 41 | 51 | 30,35 | 86 | | N | 1 | Cloudy. |
| 16 | 35 | 7 | 0 | 36 | 49 | 30,32 | 90 | | NNE | 1 | Cloudy. |
| | 41 | 2 | 0 | 41 | 51 | 30,30 | 85 | | NNE | 1 | Cloudy. |

## METEOROLOGICAL JOURNAL
### for February, 1804.

| 1804 | Sir's Therm. least and greatest Heat. | Time. H. | M. | Therm. without. ° | Therm. within. ° | Barom. Inches. | Hygrometer. | Rain. Inches. | Winds. Points. | Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Feb. 17 | 29 | 7 | 0 | 30 | 48 | 30,32 | 88 | | NNE | 1 | Fine. |
| | 38 | 2 | 0 | 37 | 51 | 30,33 | 74 | | NNE | 1 | Fair. |
| 18 | 34 | 7 | 0 | 38 | 48 | 30,20 | 89 | | N | 2 | Cloudy. |
| | 43 | 2 | 0 | 42 | 52 | 30,31 | 77 | | NE | 2 | Fair. |
| 19 | 33 | 7 | 0 | 35 | 49 | 30,33 | 91 | | NE | 1 | Cloudy. |
| | 43 | 2 | 0 | 43 | 51 | 30,28 | 87 | | NNE | 1 | Cloudy. |
| 20 | 38 | 7 | 0 | 38 | 50 | 30,34 | 92 | | NE | 1 | Cloudy. |
| | 44 | 2 | 0 | 44 | 52 | 30,37 | 85 | | NE | 1 | Cloudy. |
| 21 | 38 | 7 | 0 | 38 | 50 | 29,48 | 82 | | NNE | 1 | Cloudy. |
| | 46 | 2 | 0 | 46 | 53 | 30,51 | 79 | | N | 1 | Cloudy. |
| 22 | 42 | 7 | 0 | 42 | 52 | 30,46 | 82 | | N | 1 | Cloudy. |
| | 46 | 2 | 0 | 46 | 53 | 30,41 | 78 | | NNW | 1 | Cloudy. |
| 23 | 38 | 7 | 0 | 38 | 51 | 30,25 | 86 | | NNW | 1 | Fine. |
| | 48 | 2 | 0 | 47 | 53 | 30,16 | 73 | | NW | 1 | Fair. |
| 24 | 32 | 7 | 0 | 32 | 51 | 30,20 | 75 | | WNW | 1 | Cloudy. |
| | 45 | 2 | 0 | 45 | 52 | 29,83 | 78 | | NW | 2 | Cloudy. |
| 25 | 31 | 7 | 0 | 31 | 50 | 29,87 | 78 | 0,070 | NW | 2 | Cloudy. |
| | 38 | 2 | 0 | 38 | 52 | 29,84 | 72 | | NNW | 1 | Fair. |
| 26 | 28 | 7 | 0 | 29 | 49 | 30,03 | 80 | | NW | 1 | Fine. |
| | 41 | 2 | 0 | 41 | 52 | 30,05 | 76 | | NW | 1 | Cloudy. |
| 27 | 39 | 7 | 0 | 40 | 50 | 29,91 | 91 | | W | 1 | Cloudy. |
| | 47 | 2 | 0 | 47 | 52 | 29,92 | 82 | | NW | 1 | Cloudy. |
| 28 | 37 | 7 | 0 | 37 | 50 | 29,91 | 87 | | W | 1 | Cloudy. |
| | 44 | 2 | 0 | 38 | 52 | 29,93 | 80 | | NNE | 1 | Cloudy. |
| 29 | 29 | 7 | 0 | 29 | 49 | 29,95 | 83 | | NNE | 2 | Fine. |
| | 38 | 2 | 0 | 38 | 52 | 30,01 | 77 | | NE | 2 | Fair. |

## METEOROLOGICAL JOURNAL

### for March, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | M. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. Points. | Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | | | Inches. | | Inches. | | | |
| Mar. 1 | 29 | 7 | 0 | 30 | 49 | 29.92 | 85 | | WNW | 1 | Cloudy. |
| | 36 | 2 | 0 | 36 | 52 | 29.91 | 74 | | E | 1 | Fair. |
| 2 | 28 | 7 | 0 | 28 | 48 | 29.91 | 85 | | NE | 1 | Fine. |
| | 35 | 2 | 0 | 32 | 50 | 29.88 | 86 | | NE | 2 | Fair. |
| 3 | 29 | 7 | 0 | 32 | 48 | 29.65 | 78 | | W | 1 | Cloudy. |
| | 41 | 2 | 0 | 38 | 50 | 29.50 | 84 | | SE | 1 | Cloudy. |
| 4 | 34 | 7 | 0 | 34 | 48 | 29.42 | 90 | 0,085 | ESE | 2 | Cloudy. |
| | 37 | 2 | 0 | 36 | 49 | 29.40 | 90 | | E | 2 | Snow. |
| 5 | 36 | 7 | 0 | 39 | 48 | 29.35 | 88 | | E | 1¼ | Cloudy. |
| | 46 | 2 | 0 | 46 | 51 | 29.32 | 84 | | E | 1 | Cloudy. |
| 6 | 39 | 7 | 0 | 39 | 48 | 29.30 | 92 | 0,128 | ESE | 1 | Fair. |
| | 46 | 2 | 0 | 46 | 51 | 29.33 | 82 | | SE | 2 | Cloudy. |
| 7 | 37 | 7 | 0 | 38 | 49 | 29.58 | 91 | 0,185 | SSE | 1 | Cloudy. |
| | 50 | 2 | 0 | 50 | 52 | 29.68 | 81 | | SSE | 1 | Fair. |
| 8 | 37 | 7 | 0 | 37 | 50 | 29.73 | 90 | | E | 1 | Fair. |
| | 51 | 2 | 0 | 51 | 54 | 29.67 | 78 | | ESE | 2 | Fair. |
| 9 | 45 | 7 | 0 | 46 | 52 | 29.53 | 88 | | E | 1 | Cloudy. |
| | 52 | 2 | 0 | 51 | 53 | 29.47 | 91 | | ESE | 1 | Rain. |
| 10 | 45 | 7 | 0 | 45 | 54 | 29.76 | 95 | | S | 1 | Cloudy. |
| | 55 | 2 | 0 | 54 | 56 | 29.82 | 91 | | S | 1 | Fine. |
| 11 | 45 | 7 | 0 | 46 | 54 | 29.78 | 96 | | E | 1 | Foggy. |
| | 54 | 2 | 0 | 54 | 57 | 29.71 | 86 | | E | 1 | Fine. |
| 12 | 43 | 7 | 0 | 43 | 55 | 29.68 | 90 | | E | 1 | Fine. |
| | 57 | 2 | 0 | 57 | 58 | 29.78 | 82 | | SE | 1 | Fine. |
| 13 | 46 | 7 | 0 | 47 | 56 | 29.82 | 90 | | E | 1 | Cloudy. |
| | 59 | 2 | 0 | 59 | 58 | 29.85 | 73 | | SE | 1 | Fair. |
| 14 | 44 | 7 | 0 | 44 | 56 | 29.86 | 83 | | E | 1 | Fine. |
| | 62 | 2 | 0 | 62 | 59 | 29.85 | 68 | | S | 1 | Fine. |
| 15 | 47 | 7 | 0 | 48 | 58 | 29.80 | 79 | | SE | 1 | Cloudy. |
| | 62 | 2 | 0 | 62 | 60 | 29.75 | 68 | | S | 1 | Fair. |
| 16 | 45 | 7 | 0 | 45 | 58 | 29.76 | 91 | | SE | 1 | Foggy. |
| | 58 | 2 | 0 | 58 | 60 | 29.75 | 83 | | SE | 1 | Hazy. |

## METEOROLOGICAL JOURNAL

### for March, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|
| | | H. M. | | | Inches. | | Inches. | Points. | Str. | |
| | ° | | ° | ° | | | | | | |
| Mar. 17 | 46 | 7 0 | 46 | 58 | 29,74 | 91 | 0,028 | E | 1 | Cloudy. |
| | 58 | 2 0 | 58 | 60 | 29,68 | 85 | | E | 1 | Cloudy. |
| 18 | 44 | 7 0 | 44 | 58 | 29,55 | 92 | 0,135 | NE | 1 | Rain. |
| | 44 | 2 0 | 41 | 59 | 29,63 | 93 | | NE | 1 | Rain. |
| 19 | 36 | 7 0 | 36 | 57 | 29,58 | 92 | 0,238 | N | 1 | Snow. |
| | 40 | 2 0 | 39 | 56 | 29,53 | 85 | | NNE | 1 | Rain. |
| 20 | 34 | 7 0 | 34 | 55 | 29,58 | 86 | 0,022 | NE | 1 | Cloudy. |
| | 35 | 2 0 | 34 | 55 | 29,66 | 83 | | NE | 2 | Cloudy. |
| 21 | 32 | 7 0 | 32 | 53 | 29,87 | 82 | | NE | 2 | Cloudy. |
| | 38 | 2 0 | 38 | 54 | 29,89 | 76 | | NE | 2 | Fine. |
| 22 | 30 | 7 0 | 31 | 52 | 29,81 | 80 | | NE | 2 | Cloudy. |
| | 39 | 2 0 | 39 | 54 | 29,74 | 72 | | ENE | 2 | Fine. |
| 23 | 32 | 7 0 | 32 | 52 | 29,67 | 78 | | NE | 2 | Cloudy. |
| | 42 | 2 0 | 42 | 52 | 29,66 | 73 | | NE | 1 | Fair. |
| 24 | 33 | 7 0 | 34 | 51 | 29,72 | 85 | | NW | 1 | Fine. |
| | 46 | 2 0 | 46 | 54 | 29,81 | 68 | | NW | 1 | Fine. |
| 25 | 37 | 7 0 | 43 | 52 | 29,61 | 79 | | SW | 2 | Cloudy. |
| | 49 | 2 0 | 48 | 54 | 29,43 | 78 | | SW | 2 | Cloudy. |
| 26 | 42 | 7 0 | 44 | 51 | 29,27 | 91 | 0,065 | S | 2 | Cloudy. |
| | 50 | 2 0 | 50 | 54 | 29,15 | 81 | | SSE | 2 | Cloudy. |
| 27 | 36 | 7 0 | 39 | 52 | 29,22 | 89 | 0,300 | SSW | 2 | Cloudy. |
| | 49 | 2 0 | 47 | 55 | 29,31 | 71 | | SSW | 2 | Cloudy. |
| 28 | 38 | 7 0 | 38 | 52 | 29,54 | 91 | | NE | 2 | Cloudy. |
| | 44 | 2 0 | 43 | 53 | 29,64 | 86 | | NE | 1 | Cloudy. |
| 29 | 33 | 7 0 | 35 | 52 | 29,78 | 87 | | NE | 1 | Fair. |
| | 51 | 2 0 | 51 | 55 | 29,76 | 77 | | W | 1 | Cloudy. |
| 30 | 45 | 7 0 | 47 | 53 | 29,63 | 89 | | W | 1 | Cloudy. |
| | 56 | 2 0 | 55 | 56 | 29,58 | 79 | | W | 2 | Cloudy. |
| 31 | 43 | 7 0 | 44 | 53 | 29,81 | 93 | 0,353 | W | 1 | Rain. |
| | 49 | 2 0 | 49 | 56 | 29,18 | 78 | | WNW | 1 | Cloudy. This day there has been snow, hail, rain, and thunder and lightning. |

## METEOROLOGICAL JOURNAL

### for April, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | M. | Therm. without. ° | Therm. within. ° | Barom. Inches. | Hygrometer. | Rain. Inches. | Winds. Points. | Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| April 1 | 36 | 7 | 0 | 36 | 53 | 29,33 | 80 | 0,345 | WNW | 2 | Fair. |
| | 45 | 2 | 0 | 44 | 55 | 29,39 | 70 | | NW | 2 | Fair. |
| 2 | 35 | 7 | 0 | 37 | 52 | 29,29 | 81 | | WNW | 1 | Cloudy. |
| | 47 | 2 | 0 | 47 | 55 | 29,22 | 69 | | NE | 1 | Hazy. |
| 3 | 34 | 7 | 0 | 38 | 52 | 29,35 | 87 | | NE | 1 | Cloudy. |
| | 48 | 2 | 0 | 47 | 54 | 29,51 | 71 | | NW | 1 | Cloudy. |
| 4 | 36 | 7 | 0 | 39 | 52 | 29,67 | 85 | | W | 1 | Fine. |
| | 52 | 2 | 0 | 50 | 55 | 29,59 | 79 | | SSW | 2 | Cloudy. |
| 5 | 38 | 7 | 0 | 40 | 52 | 29,49 | 83 | | SSW | 2 | Cloudy. |
| | 48 | 2 | 0 | 46 | 55 | 29,47 | 77 | | W | 2 | Cloudy. |
| 6 | 37 | 7 | 0 | 39 | 52 | 30,00 | 80 | 0,100 | WNW | 1 | Fine. |
| | 52 | 2 | 0 | 51 | 55 | 30,14 | 70 | | NW | 2 | Fair. |
| 7 | 41 | 7 | 0 | 41 | 53 | 30,25 | 78 | | NW | 1 | Cloudy. |
| | 53 | 2 | 0 | 53 | 56 | 30,28 | 67 | | N | 1 | Cloudy. |
| 8 | 41 | 7 | 0 | 42 | 53 | 30,27 | 84 | | E | 1 | Fine. |
| | 53 | 2 | 0 | 52 | 56 | 30,24 | 65 | | E | 1 | Cloudy. |
| 9 | 37 | 7 | 0 | 39 | 54 | 30,14 | 85 | | NE | 1 | Cloudy. |
| | 49 | 2 | 0 | 49 | 56 | 30,03 | 76 | | NE | 1 | Cloudy. |
| 10 | 41 | 7 | 0 | 41 | 53 | 29,89 | 90 | 0,922 | NE | 1 | Rain. |
| | 45 | 2 | 0 | 45 | 55 | 29,92 | 80 | | NE | 1 | Cloudy. |
| 11 | 39 | 7 | 0 | 40 | 53 | 29,96 | 87 | | NE | 1 | Cloudy. |
| | 43 | 2 | 0 | 43 | 55 | 29,95 | 82 | | NE | 1 | Cloudy. |
| 12 | 39 | 7 | 0 | 41 | 53 | 29,87 | 90 | | NE | 1 | Cloudy. |
| | 44 | 2 | 0 | 44 | 55 | 29,85 | 85 | | NE | 1 | Cloudy. |
| 13 | 39 | 7 | 0 | 40 | 53 | 29,74 | 80 | | NE | 1 | Cloudy. |
| | 45 | 2 | 0 | 45 | 55 | 29,78 | 80 | | ENE | 1 | Cloudy. |
| 14 | 41 | 7 | 0 | 42 | 53 | 29,69 | 89 | | NE | 1 | Cloudy. |
| | 54 | 2 | 0 | 54 | 56 | 29,67 | 74 | | ENE | 1 | Fair. |
| 15 | 42 | 7 | 0 | 44 | 54 | 29,62 | 90 | | NE | 1 | Cloudy. |
| | 58 | 2 | 0 | 58 | 57 | 29,58 | 80 | | NE | 1 | Cloudy. |
| 16 | 46 | 7 | 0 | 46 | 55 | 29,58 | 86 | | NE | 2 | Cloudy. |
| | 50 | 2 | 0 | 50 | 57 | 29,58 | 80 | | NE | 2 | Cloudy. |

## METEOROLOGICAL JOURNAL

### for April, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | Time. M. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. Points. | Winds. Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | ° | ° | Inches. | | Inches. | | | |
| Apr. 17 | 43 | 7 | 0 | 44 | 55 | 29.62 | 86 | | NE | 2 | Cloudy. |
| | 47 | 2 | 0 | 46 | 57 | 29.62 | 86 | | NE | 2 | Cloudy. |
| 18 | 41 | 7 | 0 | 42 | 55 | 29.70 | 88 | 0,075 | NE | 2 | Cloudy. |
| | 49 | 2 | 0 | 48 | 56 | 29.77 | 73 | | NE | 2 | Cloudy. |
| 19 | 34 | 7 | 0 | 36 | 55 | 29.87 | 81 | | N | 2 | Fine. |
| | 47 | 2 | 0 | 44 | 56 | 29.80 | 72 | | NW | 2 | Cloudy. |
| 20 | 35 | 7 | 0 | 36 | 54 | 29.60 | 83 | 0,095 | NW | 2 | Cloudy. |
| | 45 | 2 | 0 | 43 | 56 | 29.67 | 82 | | NW | 2 | Cloudy. |
| 21 | 36 | 7 | 0 | 37 | 54 | 29.64 | 77 | 0,055 | NW | 2 | Fine. |
| | 48 | 2 | 0 | 48 | 56 | 29.66 | 65 | | NW | 2 | Hazy. |
| 22 | 34 | 7 | 0 | 37 | 54 | 29.65 | 86 | | WNW | 1 | Fine. |
| | 51 | 2 | 0 | 50 | 56 | 29.66 | 65 | | WSW | 1 | Hazy. |
| 23 | 35 | 7 | 0 | 42 | 53 | 29.55 | 90 | 0,155 | SW | 2 | Cloudy. |
| | 49 | 2 | 0 | 47 | 57 | 29.61 | 87 | | W | 1 | Rain. |
| 24 | 44 | 7 | 0 | 50 | 55 | 29.62 | 94 | 0,390 | S | 2 | Rain. |
| | 58 | 2 | 0 | 58 | 57 | 29.67 | 81 | | S | 2 | Cloudy. |
| 25 | 51 | 7 | 0 | 52 | 56 | 29.51 | 90 | 0,050 | E | 1 | Cloudy. |
| | 56 | 2 | 0 | 52 | 58 | 29.44 | 84 | | S | 2 | Rain. |
| 26 | 47 | 7 | 0 | 49 | 56 | 29.45 | 87 | 0,062 | SSW | 2 | Cloudy. |
| | 58 | 2 | 0 | 58 | 59 | 29.55 | 75 | | W | 2 | Cloudy. |
| 27 | 50 | 7 | 0 | 53 | 58 | 29.34 | 93 | 0,190 | SSW | 2 | Cloudy. |
| | 61 | 2 | 0 | 61 | 61 | 29.49 | 72 | | WSW | 2 | Fair. |
| 28 | 50 | 7 | 0 | 51 | 59 | 29.61 | 87 | | SSE | 1 | Cloudy. |
| | 60 | 2 | 0 | 60 | 60 | 29.66 | 86 | | S | 1 | Rain. |
| 29 | 52 | 7 | 0 | 53 | 59 | 29.88 | 90 | 0,027 | SSW | 1 | Cloudy. |
| | 66 | 2 | 0 | 66 | 61 | 29.92 | 71 | | SSW | 1 | Fair. |
| 30 | 52 | 7 | 0 | 53 | 60 | 29.90 | 88 | 0,038 | SSE | 1 | Cloudy. |
| | 71 | 2 | 0 | 70 | 61 | 29.86 | 75 | | SE | 1 | Fair. |

## METEOROLOGICAL JOURNAL
### for May, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | M. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. Points. | Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | ° | ° | Inches. | | Inches. | | | |
| May 1 | 56 | 7 | 0 | 58 | 62 | 29,80 | 87 | | ESE | 1 | Cloudy. |
| | 71 | 2 | 0 | 70 | 63 | 29,78 | 76 | | SW | 1 | Hazy. |
| 2 | 52 | 7 | 0 | 53 | 62 | 29,92 | 86 | | SW | 1 | Fine. |
| | 68 | 2 | 0 | 68 | 64 | 29,93 | 71 | | E | 1 | Fair. |
| 3 | 53 | 7 | 0 | 53 | 62 | 29,80 | 86 | | E | 2 | Cloudy. |
| | 71 | 2 | 0 | 70 | 64 | 29,82 | 77 | | ESE | 2 | Fair. |
| 4 | 58 | 7 | 0 | 60 | 63 | 29,86 | 86 | | NW | 1 | Cloudy. |
| | 71 | 2 | 0 | 70 | 64 | 29,84 | 79 | | NNE | 1 | Cloudy. |
| 5 | 56 | 7 | 0 | 58 | 63 | 29,84 | 92 | 0,016 | NNE | 1 | Cloudy. |
| | 73 | 2 | 0 | 73 | 66 | 29,98 | 70 | | ESE | 1 | Fair. |
| 6 | 58 | 7 | 0 | 60 | 65 | 30,17 | 88 | | SSE | 1 | Cloudy. |
| | 70 | 2 | 0 | 70 | 65 | 30,20 | 75 | | SW | 1 | Cloudy. |
| 7 | 58 | 7 | 0 | 61 | 65 | 30,27 | 87 | | W | 1 | Cloudy. |
| | 72 | 2 | 0 | 71 | 67 | 30,27 | 73 | | NW | 1 | Fair. |
| 8 | 53 | 7 | 0 | 57 | 66 | 30,29 | 81 | | NE | 1 | Fair. |
| | 65 | 2 | 0 | 65 | 67 | 30,22 | 68 | | NE | 1 | Fair. |
| 9 | 50 | 7 | 0 | 53 | 64 | 30,12 | 81 | | NE | 1 | Cloudy. |
| | 67 | 2 | 0 | 67 | 65 | 29,99 | 69 | | SSW | 1 | Fair. |
| 10 | 53 | 7 | 0 | 53 | 63 | 29,91 | 87 | 0,178 | WNW | 1 | Fair. |
| | 63 | 2 | 0 | 62 | 64 | 29,93 | 67 | | NW | 1 | Cloudy. |
| 11 | 49 | 7 | 0 | 50 | 63 | 29,93 | 80 | | NW | 1 | Fine. |
| | 62 | 2 | 0 | 59 | 64 | 29,94 | 67 | | NW | 1 | Cloudy. |
| 12 | 48 | 7 | 0 | 42 | 61 | 30,25 | 72 | 0,075 | NNW | 1 | Fair. |
| | 58 | 2 | 0 | 57 | 62 | 30,26 | 66 | | NNW | 1 | Fair. |
| 13 | 44 | 7 | 0 | 47 | 61 | 30,28 | 78 | | W | 1 | Fine. |
| | 64 | 2 | 0 | 62 | 63 | 30,28 | 64 | | NE | 1 | Fair. |
| 14 | 47 | 7 | 0 | 52 | 60 | 30,03 | 74 | | E | 1 | Hazy. |
| | 63 | 2 | 0 | 64 | 62 | 30,07 | 70 | | SSW | 1 | Fair. |
| 15 | 50 | 7 | 0 | 53 | 61 | 30,06 | 75 | | SSE | 1 | Cloudy. |
| | 70 | 2 | 0 | 70 | 62 | 30,02 | 68 | | S | 1 | Fair. |
| 16 | 56 | 7 | 0 | 60 | 62 | 29,88 | 77 | | SSE | 1 | Cloudy. |
| | 69 | 2 | 0 | 68 | 62 | 29,78 | 72 | | SE | 1 | Cloudy. |

## METEOROLOGICAL JOURNAL
### for May, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | M. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. Points. | Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | ° | ° | Inches. | | Inches. | | | |
| May 17 | 53 | 7 | 0 | 53 | 62 | 29.67 | 88 | 0.373 | SW | 1 | Fair. |
| | 67 | 2 | 0 | 67 | 63 | 29.78 | 67 | | W | 1 | Fair. |
| 18 | 51 | 7 | 0 | 54 | 62 | 30.03 | 86 | | SSW | 1 | Cloudy. |
| | 66 | 2 | 0 | 66 | 63 | 30.01 | 70 | | S | 1 | Fair. |
| 19 | 54 | 7 | 0 | 56 | 62 | 29.86 | 81 | 0.033 | SE | 1 | Cloudy. |
| | 68 | 2 | 0 | 68 | 63 | 29.77 | 69 | | SE | 1 | Hazy. |
| 20 | 52 | 7 | 0 | 57 | 62 | 29.74 | 77 | | NNE | 1 | Fair. |
| | 70 | 2 | 0 | 69 | 64 | 29.77 | 67 | | NE | 2 | Fair. |
| 21 | 51 | 7 | 0 | 54 | 62 | 29.83 | 73 | | ENE | 2 | Fair. |
| | 66 | 2 | 0 | 65 | 64 | 29.73 | 71 | | E | 2 | Fair. |
| 22 | 56 | 7 | 0 | 58 | 63 | 29.78 | 90 | 0.058 | SE | 2 | Rain. |
| | 65 | 2 | 0 | 63 | 63 | 29.85 | 73 | | W | 2 | Cloudy. |
| 23 | 52 | 7 | 0 | 53 | 62 | 30.07 | 86 | | SSW | 1 | Cloudy. |
| | 67 | 2 | 0 | 67 | 63 | 30.07 | 69 | | S | 1 | Fair. |
| 24 | 55 | 7 | 0 | 56 | 62 | 29.80 | 88 | 0.047 | S | 2 | Rain. |
| | 61 | 2 | 0 | 61 | 62 | 29.64 | 87 | | S | 2 | Rain. |
| 25 | 51 | 7 | 0 | 52 | 61 | 29.68 | 82 | 0.030 | SW | 2 | Fair. |
| | 65 | 2 | 0 | 65 | 62 | 29.75 | 70 | | SW | 2 | Fair. |
| 26 | 54 | 7 | 0 | 54 | 62 | 29.67 | 87 | | S | 2 | Fine. |
| | 67 | 2 | 0 | 67 | 62 | 29.69 | 67 | | S | 2 | Hazy. |
| 27 | 51 | 7 | 0 | 53 | 61 | 29.86 | 82 | | S | 2 | Fair. |
| | 66 | 2 | 0 | 65 | 62 | 29.90 | 68 | | S | 2 | Fair. |
| 28 | 52 | 7 | 0 | 56 | 61 | 29.86 | 80 | | S | 2 | Fair. |
| | 65 | 2 | 0 | 63 | 62 | 29.78 | 69 | | SSE | 2 | Cloudy. |
| 29 | 54 | 7 | 0 | 55 | 61 | 29.71 | 87 | 0.333 | W | 2 | Cloudy. |
| | 66 | 2 | 0 | 66 | 62 | 29.91 | 70 | | WNW | 2 | Fair. |
| 30 | 48 | 7 | 0 | 51 | 60 | 30.10 | 84 | 0.045 | W | 2 | Fine. |
| | 64 | 2 | 0 | 64 | 61 | 30.06 | 72 | | S | 2 | Fair. |
| 31 | 53 | 7 | 0 | 53 | 61 | 29.96 | 81 | | WNW | 1 | Fine. |
| | 68 | 2 | 0 | 68 | 62 | 29.99 | 68 | | WNW | 2 | Fair. |

## METEOROLOGICAL JOURNAL
### for June, 1804.

| 1804 | Six'y Therm. least and greatest Heat. | Time. H. | M. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. Points. | Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | ° | ° | Inches. | ° | Inches. | | | |
| June 1 | 51 | 7 | 0 | 55 | 61 | 30,14 | 82 | | WSW | 1 | Fair. |
| | 70 | 2 | 0 | 70 | 63 | 30,14 | 67 | | WSW | 1 | Fair. |
| 2 | 53 | 7 | 0 | 55 | 61 | 30,12 | 83 | | WSW | 2 | Cloudy. |
| | 75 | 2 | 0 | 74 | 63 | 30,06 | 62 | | SSW | 1 | Fine. |
| 3 | 56 | 7 | 0 | 58 | 63 | 30,02 | 81 | | SW | 1 | Fine. |
| | 79 | 2 | 0 | 78 | 66 | 30,01 | 62 | | S | 1 | Fine. |
| 4 | 60 | 7 | 0 | 66 | 65 | 29,97 | 76 | | E | 1 | Cloudy. |
| | 81 | 2 | 0 | 80 | 71 | 29,94 | 72 | | S | 1 | Fine. |
| 5 | 60 | 7 | 0 | 61 | 67 | 30,03 | 81 | | W | 1 | Cloudy. |
| | 75 | 2 | 0 | 75 | 68 | 30,04 | 72 | | WSW | 1 | Cloudy. |
| 6 | 61 | 7 | 0 | 63 | 67 | 30,02 | 82 | | WSW | 1 | Cloudy. |
| | 71 | 2 | 0 | 70 | 68 | 29,94 | 73 | | S | 2 | Cloudy. |
| 7 | 54 | 7 | 0 | 56 | 66 | 29,91 | 80 | | S | 2 | Cloudy. |
| | 69 | 2 | 0 | 69 | 68 | 29,79 | 68 | | SSW | 2 | Cloudy. |
| 8 | 51 | 7 | 0 | 55 | 65 | 29,69 | 78 | | SW | 2 | Cloudy. |
| | 66 | 2 | 0 | 64 | 66 | 29,68 | 70 | | SW | 2 | Cloudy. |
| 9 | 53 | 7 | 0 | 53 | 64 | 29,94 | 77 | | W | 2 | Fair. |
| | 67 | 2 | 0 | 67 | 66 | 30,04 | 66 | | WNW | 2 | Fair. |
| 10 | 50 | 7 | 0 | 53 | 64 | 30,12 | 82 | | SW | 1 | Cloudy. |
| | 63 | 2 | 0 | 63 | 64 | 30,10 | 82 | | WSW | 1 | Cloudy. |
| 11 | 47 | 7 | 0 | 50 | 63 | 30,36 | 79 | 0,260 | NE | 1 | Fine. |
| | 66 | 2 | 0 | 65 | 64 | 30,38 | 68 | | NE | 1 | Fair. |
| 12 | 49 | 7 | 0 | 53 | 63 | 30,40 | 83 | | NE | 1 | Fair. |
| | 70 | 2 | 0 | 70 | 65 | 30,40 | 70 | | ENE | 1 | Fine. |
| 13 | 57 | 7 | 0 | 58 | 63 | 30,36 | 76 | | ESE | 1 | Cloudy. |
| | 70 | 2 | 0 | 70 | 64 | 30,31 | 73 | | ESE | 1 | Cloudy. |
| 14 | 61 | 7 | 0 | 62 | 64 | 30,20 | 83 | | E | 1 | Cloudy. |
| | 69 | 2 | 0 | 64 | 64 | 30,13 | 83 | | N | 1 | Rain. |
| 15 | 57 | 7 | 0 | 57 | 64 | 30,00 | 87 | 0,262 | NE | 1 | Fine. |
| | 70 | 2 | 0 | 69 | 65 | 29,94 | 72 | | NE | 1 | Cloudy. |
| 16 | 52 | 7 | 0 | 54 | 64 | 29,83 | 84 | | E | 1 | Cloudy. |
| | 70 | 2 | 0 | 69 | 66 | 29,76 | 78 | | E | 1 | Fair. |

## METEOROLOGICAL JOURNAL
### for June, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | M. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. Points. | Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | ° | ° | Inches. | ° | Inches. | | | |
| June 17 | 56 | 7 | 0 | 58 | 64 | 29,83 | 87 | | SW | 1 | Fine. |
| | 72 | 2 | 0 | 71 | 66 | 29,92 | 69 | | WNW | 1 | Cloudy. |
| 18 | 57 | 7 | 0 | 58 | 65 | 30,10 | 83 | | W | 1 | Fine. |
| | 73 | 2 | 0 | 72 | 66 | 30,19 | 70 | | W | 1 | Fine. |
| 19 | 55 | 7 | 0 | 57 | 63 | 30,37 | 80 | | SW | 1 | Hazy. |
| | 68 | 2 | 0 | 68 | 65 | 30,36 | 73 | | SSW | 1 | Cloudy. |
| 20 | 60 | 7 | 0 | 62 | 65 | 30,34 | 83 | | SW | 2 | Cloudy. |
| | 76 | 2 | 0 | 75 | 68 | 30,33 | 72 | | WNW | 1 | Fair. |
| 21 | 58 | 7 | 0 | 60 | 66 | 30,36 | 88 | | WSW | 1 | Cloudy. |
| | 81½ | 2 | 0 | 81 | 68 | 30,31 | 71 | | SSW | 1 | Fine. |
| 22 | 61 | 7 | 0 | 64 | 68 | 30,33 | 81 | | WNW | 1 | Fine. |
| | 78 | 2 | 0 | 77 | 70 | 30,30 | 70 | | NE | 1 | Hazy. |
| 23 | 56 | 7 | 0 | 58 | 68 | 30,34 | 77 | | E | 1 | Fine. |
| | 68 | 2 | 0 | 68 | 68 | 30,30 | 70 | | E | 1 | Fine. |
| 24 | 56 | 7 | 0 | 60 | 68 | 30,25 | 83 | | E | 1 | Fine. |
| | 75 | 2 | 0 | 75 | 69 | 30,21 | 66 | | E | 1 | Fine. |
| 25 | 59 | 7 | 0 | 63 | 68 | 30,18 | 79 | | ENE | 1 | Fine. |
| | 87 | 2 | 0 | 85 | 71 | 30,17 | 63 | | SE | 1 | Fair. |
| 26 | 57 | 7 | 0 | 60 | 68 | 30,24 | 83 | | NE | 1 | Cloudy. |
| | 68 | 2 | 0 | 68 | 69 | 30,20 | 75 | | E | 1 | Cloudy. |
| 27 | 56 | 7 | 0 | 58 | 68 | 30,04 | 77 | | NE | 1 | Fair. |
| | 68 | 2 | 0 | 67 | 68 | 30,00 | 70 | | NE | 1 | Fair. |
| 28 | 49 | 7 | 0 | 53 | 67 | 30,11 | 78 | | NE | 2 | Fair. |
| | 63 | 2 | 0 | 61 | 67 | 30,16 | 71 | | NE | 2 | Cloudy. |
| 29 | 48 | 7 | 0 | 52 | 65 | 30,14 | 78 | | W | 1 | Fair. |
| | 72 | 2 | 0 | 72 | 67 | 30,02 | 64 | | WSW | 1 | Fine. |
| 30 | 54 | 7 | 0 | 56 | 66 | 29,97 | 77 | | WSW | 1 | Fair. |
| | 74 | 2 | 0 | 74 | 67 | 29,90 | 66 | | SW | 1 | Fine. |

## METEOROLOGICAL JOURNAL

### for July, 1804.

| 1804 | Six's Therm. least, and greatest Heat. | Time. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|
| | | H. M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | ° | | | | |
| July 1 | 58 | 7 0 | 60 | 66 | 29,81 | 73 | | SSE | 1 | Fine. |
| | 73 | 2 0 | 73 | 68 | 29,82 | 67 | | SSE | 1 | Fine. |
| 2 | 56 | 7 0 | 61 | 67 | 29,86 | 80 | | E | 1 | Fair. |
| | 72 | 2 0 | 72 | 68 | 29,80 | 67 | | E | 1 | Fair. |
| 3 | 57 | 7 0 | 62 | 67 | 29,70 | 78 | | ENE | 1 | Cloudy. |
| | 65 | 2 0 | 65 | 68 | 29,68 | 81 | | NW | 1 | Cloudy. |
| 4 | 54 | 7 0 | 55 | 65 | 29,82 | 82 | 0,032 | NW | 2 | Fine. |
| | 71 | 2 0 | 71 | 67 | 29,85 | 63 | | NW | 1 | Fine. |
| 5 | 55 | 7 0 | 56 | 63 | 29,82 | 79 | | SW | 1 | Rain. |
| | 65 | 2 0 | 65 | 65 | 29,75 | 72 | | SSE | 1 | Cloudy. |
| 6 | 54 | 7 0 | 55 | 65 | 29,71 | 90 | 0,200 | NE | 1 | Rain. |
| | 63 | 2 0 | 63 | 65 | 29,83 | 85 | | ESE | 1 | Cloudy. |
| 7 | 57 | 7 0 | 60 | 65 | 29,83 | 90 | 0,075 | SE | 1 | Cloudy. |
| | 63 | 2 0 | 62 | 65 | 29,75 | 90 | | S | 1 | Rain. |
| 8 | 56 | 7 0 | 58 | 64 | 29,68 | 90 | 0,415 | SW | 1 | Fair. |
| | 71 | 2 0 | 71 | 66 | 29,68 | 71 | | SW | 1 | Fair. |
| 9 | 55 | 7 0 | 57 | 65 | 29,78 | 87 | 0,105 | WSW | 1 | Cloudy. |
| | 73 | 2 0 | 73 | 66 | 29,77 | 71 | | SW | 1 | Fair. |
| 10 | 58 | 7 0 | 58 | 65 | 29,74 | 86 | 0,035 | E | 1 | Rain. |
| | 62 | 2 0 | 62 | 65 | 29,64 | 94 | | E | 1 | Rain. |
| 11 | 52 | 7 0 | 53 | 64 | 29,98 | 81 | 0,090 | NNE | 2 | Fine. |
| | 60 | 2 0 | 60 | 64 | 30,11 | 67 | | NE | 1 | Cloudy. |
| 12 | 50 | 7 0 | 53 | 63 | 30,23 | 71 | | N | 1 | Cloudy. |
| | 64 | 2 0 | 63 | 64 | 30,21 | 64 | | N | 1 | Cloudy. |
| 13 | 49 | 7 0 | 53 | 63 | 30,21 | 77 | | NE | 1 | Fair. |
| | 68 | 2 0 | 68 | 65 | 30,17 | 63 | | NE | 1 | Fair. |
| 14 | 53 | 7 0 | 57 | 62 | 30,16 | 78 | | E | 1 | Cloudy. |
| | 60 | 2 0 | 60 | 64 | 30,10 | 69 | | E | 1 | Cloudy. |
| 15 | 51 | 7 0 | 53 | 63 | 30,08 | 82 | | E | 1 | Fine. |
| | 73 | 2 0 | 71 | 65 | 30,04 | 65 | | ENE | 1 | Fine. |
| 16 | 58 | 7 0 | 60 | 64 | 30,11 | 80 | | NE | 1 | Fine. |
| | 78 | 2 0 | 77 | 67 | 30,13 | 62 | | NE | 1 | Fine. |

## METEOROLOGICAL JOURNAL

### for July, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | H. | M. | | | Inches. | | Inches. | Points. | Str. | |
| | ° | | | ° | ° | | ° | | | | |
| July 17 | 61 | 7 | 0 | 64 | 66 | 30,12 | 78 | | WSW | 1 | Fair. |
| | 80 | 2 | 0 | 79 | 68 | 30,05 | 63 | | S | 2 | Fine. |
| 18 | 63 | 7 | 0 | 68 | 68 | 29,85 | 73 | | SW | 1 | Cloudy. |
| | 75 | 2 | 0 | 74 | 69 | 29,76 | 68 | | SSW | 1 | Cloudy. |
| 19 | 61 | 7 | 0 | 62 | 67 | 29,57 | 83 | 0,062 | SSW | 1 | Rain. |
| | 74 | 2 | 0 | 72 | 69 | 29,55 | 68 | | WSW | 1 | Fair. |
| 20 | 58 | 7 | 0 | 61 | 67 | 29,65 | 77 | 0,190 | NE | 1 | Fair. |
| | 67 | 2 | 0 | 63 | 67 | 29,72 | 83 | | NE | 1 | Cloudy. |
| 21 | 58 | 7 | 0 | 58 | 66 | 29,79 | 82 | | NNE | 2 | Cloudy. |
| | 67 | 2 | 0 | 67 | 68 | 29,85 | 70 | | NE | 2 | Fair. |
| 22 | 55 | 7 | 0 | 58 | 66 | 29,73 | 78 | | E | 1 | Cloudy. |
| | 69 | 2 | 0 | 67 | 67 | 29,53 | 78 | | SSW | 1 | Cloudy. |
| 23 | 55 | 7 | 0 | 57 | 66 | 29,44 | 81 | 0,053 | SSE | 2 | Fair. |
| | 68 | 2 | 0 | 67 | 66 | 29,44 | 73 | | SSE | 2 | Fair. |
| 24 | 57 | 7 | 0 | 60 | 66 | 29,62 | 80 | | ENE | 1 | Cloudy. |
| | 73 | 2 | 0 | 70 | 67 | 29,64 | 66 | | NW | 1 | Cloudy. |
| 25 | 56 | 7 | 0 | 57 | 64 | 29,61 | 75 | | WNW | 1 | Cloudy. |
| | 71 | 2 | 0 | 71 | 67 | 29,54 | 63 | | WNW | 1 | Fair. |
| 26 | 58 | 7 | 0 | 60 | 66 | 29,44 | 90 | 0,390 | NE | 1 | Rain. |
| | 70 | 2 | 0 | 70 | 67 | 29,51 | 66 | | NE | 1 | Fair. |
| 27 | 55 | 7 | 0 | 57 | 66 | 29,54 | 81 | | SW | 1 | Fine. |
| | 66 | 2 | 0 | 66 | 66 | 29,48 | 73 | | S | 2 | Cloudy. |
| 28 | 54 | 7 | 0 | 57 | 65 | 29,50 | 81 | 0,042 | S | 2 | Fair. |
| | 68 | 2 | 0 | 67 | 66 | 29,55 | 73 | | S | 2 | Fair. |
| 29 | 57 | 7 | 0 | 58 | 65 | 29,65 | 79 | | S | 2 | Fair. |
| | 72 | 2 | 0 | 72 | 67 | 29,76 | 69 | | S | 2 | Fair. |
| 30 | 55 | 7 | 0 | 56 | 65 | 30,03 | 83 | 0,016 | S | 1 | Cloudy. |
| | 76 | 2 | 0 | 75 | 68 | 30,05 | 66 | | WSW | 1 | Fine. |
| 31 | 58 | 7 | 0 | 59 | 65 | 30,20 | 75 | | W | 1 | Fine. |
| | 78 | 2 | 0 | 77 | 69 | 30,20 | 60 | | W | 1 | Fine. |

## METEOROLOGICAL JOURNAL
### for August, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|
| | | H. M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | ° | | | | |
| Aug. 1 | 68 | 7 0 | 70 | 69 | 30,27 | 78 | | NE | 1 | Cloudy. |
| | 79 | 2 0 | 79 | 70 | 30,27 | 67 | | NE | 1 | Cloudy. |
| 2 | 61 | 7 0 | 62 | 68 | 30,16 | 78 | | NE | 1 | Cloudy. |
| | 71 | 2 0 | 71 | 69 | 30,06 | 75 | | NE | 1 | Cloudy. |
| 3 | 61 | 7 0 | 63 | 68 | 29,92 | 91 | | NE | 1 | Cloudy. |
| | 81 | 2 0 | 81 | 70 | 29,83 | 74 | | ENE | 1 | Fair. |
| 4 | 64 | 7 0 | 65 | 69 | 29,78 | 87 | | NNW | 1 | Cloudy. |
| | 74 | 2 0 | 72 | 70 | 29,76 | 78 | | NW | 1 | Cloudy. |
| 5 | 57 | 7 0 | 61 | 69 | 29,84 | 84 | 0,090 | W | 1 | Cloudy. |
| | 74 | 2 0 | 73 | 70 | 29,81 | 67 | | SSW | 2 | Fair. |
| 6 | 58 | 7 0 | 59 | 66 | 29,91 | 80 | | WSW | 2 | Fair. |
| | 73 | 2 0 | 72 | 69 | 29,98 | 62 | | WNW | 1 | Fair. |
| 7 | 53 | 7 0 | 54 | 67 | 30,10 | 78 | | W | 1 | Fair. |
| | 71 | 2 0 | 71 | 68 | 30,11 | 63 | | NW | 2 | Fair. |
| 8 | 58 | 7 0 | 58 | 65 | 29,68 | 86 | 0,300 | S | 2 | Rain. |
| | 73 | 2 0 | 73 | 68 | 29,55 | 67 | | W | 2 | Fair. |
| 9 | 54 | 7 0 | 55 | 67 | 29,86 | 79 | | WSW | 1 | Fair. |
| | 73 | 2 0 | 72 | 68 | 29,81 | 65 | | S | 2 | Cloudy. |
| 10 | 58 | 7 0 | 58 | 65 | 29,56 | 80 | | S | 2 | Fair. |
| | 70 | 2 0 | 69 | 67 | 29,66 | 71 | | W | 2 | Cloudy. |
| 11 | 55 | 7 0 | 56 | 66 | 30,00 | 78 | | SSW | 2 | Fine. |
| | 70 | 2 0 | 70 | 66 | 29,91 | 65 | | SW | 2 | Cloudy. |
| 12 | 57 | 7 0 | 57 | 65 | 29,70 | 80 | 0,016 | S | 2 | Cloudy. |
| | 65 | 2 0 | 63 | 66 | 29,62 | 71 | | S | 2 | Cloudy. |
| 13 | 53 | 7 0 | 56 | 65 | 29,61 | 80 | 0,072 | S | 2 | Cloudy. |
| | 63 | 2 0 | 57 | 64 | 29,18 | 93 | | E | 1 | Rain. |
| 14 | 49 | 7 0 | 50 | 63 | 29,47 | 85 | 0,880 | W | 1 | Fine. |
| | 70 | 2 0 | 70 | 65 | 29,53 | 65 | | S | 1 | Fair. |
| 15 | 54 | 7 0 | 55 | 63 | 29,58 | 88 | | S | 1 | Cloudy. |
| | 71 | 2 0 | 71 | 65 | 29,57 | 77 | | SSW | 2 | Cloudy. |
| 16 | 57 | 7 0 | 57 | 64 | 29,57 | 91 | 1,030 | SSW | 1 | Rain. |
| | 69 | 2 0 | 68 | 66 | 29,64 | 72 | | WSW | 2 | Fair. |

## METEOROLOGICAL JOURNAL

### for August, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|
| | | H. M. | ° | ° | Inches. | ° | Inches. | Points. | Str. | |
| Aug. 17 | 54 | 7 0 | 55 | 64 | 29,80 | 83 | 0,075 | WSW | 2 | Cloudy. |
| | 68 | 2 0 | 64 | 65 | 29,87 | 73 | | WSW | 2 | Fair. |
| 18 | 50 | 7 0 | 52 | 63 | 30,00 | 80 | 0,135 | SW | 1 | Rain. |
| | 63 | 2 0 | 62 | 64 | 29,92 | 73 | | W | 1 | Cloudy. |
| 19 | 54 | 7 0 | 55 | 63 | 29,77 | 84 | 0,110 | NE | 1 | Cloudy. |
| | 67 | 2 0 | 65 | 64 | 29,80 | 69 | | N | 1 | Cloudy. |
| 20 | 51 | 7 0 | 52 | 62 | 29,87 | 82 | | W | 1 | Cloudy. |
| | 64 | 2 0 | 64 | 63 | 29,88 | 71 | | NW | 1 | Cloudy. |
| 21 | 53 | 7 0 | 54 | 62 | 29,92 | 83 | | W | 1 | Cloudy. |
| | 63 | 2 0 | 62 | 63 | 29,94 | 72 | | NW | 1 | Cloudy. |
| 22 | 56 | 7 0 | 56 | 62 | 29,97 | 77 | | NE | 1 | Cloudy. |
| | 66 | 2 0 | 66 | 64 | 30,00 | 60 | | NE | 1 | Fair. |
| 23 | 54 | 7 0 | 54 | 62 | 30,12 | 78 | | N | 1 | Cloudy. |
| | 63 | 2 0 | 63 | 63 | 30,13 | 70 | | NE | 1 | Cloudy. |
| 24 | 54 | 7 0 | 55 | 62 | 30,17 | 78 | | W | 1 | Cloudy. |
| | 66 | 2 0 | 66 | 63 | 30,17 | 67 | | WNW | 1 | Cloudy. |
| 25 | 53 | 7 0 | 55 | 62 | 30,14 | 80 | | N | 1 | Cloudy. |
| | 64 | 2 0 | 64 | 63 | 30,17 | 68 | | NNE | 1 | Cloudy. |
| 26 | 56 | 7 0 | 56 | 62 | 30,28 | 77 | | NE | 1 | Cloudy. |
| | 67 | 2 0 | 66 | 63 | 30,30 | 68 | | NE | 1 | Fair. |
| 27 | 52 | 7 0 | 52 | 62 | 30,24 | 78 | | SW | 1 | Fine. |
| | 71 | 2 0 | 70 | 64 | 30,16 | 65 | | W | 1 | Fair. |
| 28 | 59 | 7 0 | 60 | 63 | 30,17 | 83 | | WSW | 1 | Cloudy. |
| | 72 | 2 0 | 72 | 65 | 30,19 | 72 | | WSW | 1 | Fair. |
| 29 | 58 | 7 0 | 58 | 65 | 30,17 | 85 | | SW | 1 | Fine. |
| | 77 | 2 0 | 75 | 67 | 30,12 | 71 | | S | 1 | Fine. |
| 30 | 63 | 7 0 | 64 | 67 | 29,90 | 88 | 0,093 | NE | 1 | Fair. |
| | 80 | 2 0 | 80 | 69 | 29,89 | 75 | | S | 1 | Fine. |
| 31 | 63 | 7 0 | 63 | 68 | 30,07 | 80 | | NE | 1 | Fair. |
| | 73 | 2 0 | 72 | 69 | 30,09 | 73 | | NE | 1 | Cloudy. |

## METEOROLOGICAL JOURNAL

### for September, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | Time. M. | Therm. without. | Therm. within. | Barom. Inches. | Hygrometer. | Rain. Inches. | Winds. Points. | Winds. Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | ° | ° | | ° | | | | |
| Sept. 1 | 58 | 7 | 0 | 58 | 66 | 30,00 | 76 | | W | 1 | Fine. |
| | 68 | 2 | 0 | 68 | 68 | 30,10 | 63 | | WNW | 1 | Fine. |
| 2 | 54 | 7 | 0 | 55 | 67 | 30,20 | 80 | | SW | 1 | Fine. |
| | 69 | 2 | 0 | 69 | 68 | 30,18 | 67 | | SW | 1 | Fair. |
| 3 | 53 | 7 | 0 | 55 | 65 | 30,20 | 81 | | SW | 1 | Fine. |
| | 68 | 2 | 0 | 67 | 67 | 30,21 | 75 | | SSW | 2 | Cloudy. |
| 4 | 58 | 7 | 0 | 58 | 66 | 30,28 | 88 | | SW | 1 | Cloudy. |
| | 73 | 2 | 0 | 73 | 67 | 30,32 | 73 | | SSW | 1 | Cloudy. |
| 5 | 58 | 7 | 0 | 58 | 67 | 30,30 | 87 | | WNW | 1 | Fair. |
| | 73 | 2 | 0 | 73 | 68 | 30,27 | 68 | | S | 1 | Cloudy. |
| 6 | 57 | 7 | 0 | 57 | 65 | 30,13 | 85 | | NE | 1 | Fair. |
| | 72 | 2 | 0 | 72 | 70 | 30,02 | 68 | | ENE | 1 | Fine. |
| 7 | 55 | 7 | 0 | 55 | 67 | 29,94 | 84 | | E | 1 | Fine. |
| | 72 | 2 | 0 | 72 | 69 | 29,97 | 69 | | SW | 1 | Fair. |
| 8 | 53 | 7 | 0 | 55 | 67 | 30,16 | 82 | | W | 1 | Fair. |
| | 70 | 2 | 0 | 70 | 69 | 30,20 | 68 | | E | 1 | Fine. |
| 9 | 54 | 7 | 0 | 55 | 68 | 30,22 | 80 | | ENE | 1 | Fine. |
| | 70 | 2 | 0 | 70 | 69 | 30,15 | 70 | | E | 1 | Fine. |
| 10 | 52 | 7 | 0 | 55 | 66 | 30,00 | 81 | | NE | 1 | Fine. |
| | 74 | 2 | 0 | 74 | 68 | 29,98 | 73 | | S | 2 | Cloudy. |
| 11 | 57 | 7 | 0 | 57 | 67 | 30,14 | 84 | | SW | 1 | Fair. |
| | 69 | 2 | 0 | 68 | 69 | 30,17 | 66 | | SSW | 1 | Fair. |
| 12 | 55 | 7 | 0 | 57 | 68 | 30,18 | 78 | | SW | 1 | Fine. |
| | 79 | 2 | 0 | 78 | 72 | 30,11 | 73 | | SSE | 1 | Fine. |
| 13 | 62 | 7 | 0 | 64 | 70 | 30,00 | 83 | | S | 1 | Fine. |
| | 81 | 2 | 0 | 81 | 73 | 30,00 | 70 | | S | 2 | Fine. |
| 14 | 62 | 7 | 0 | 62 | 68 | 30,02 | 86 | | S | 1 | Cloudy. |
| | 79 | 2 | 0 | 79 | 73 | 29,92 | 73 | | SE | 1 | Fine. |
| 15 | 63 | 7 | 0 | 64 | 70 | 29,96 | 83 | | S | 1 | Cloudy. |
| | 78 | 2 | 0 | 78 | 73 | 29,96 | 73 | | SE | 2 | Fine. |
| 16 | 62 | 7 | 0 | 63 | 70 | 30,01 | 91 | | NE | 1 | Cloudy. |
| | 81½ | 2 | 0 | 81 | 74 | 30,07 | 70 | | E | 1 | Fine. |

## METEOROLOGICAL JOURNAL
### for September, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | Time. M. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. Points. | Winds. Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | ° | ° | Inches. | | Inches. | | | |
| Sep. 17 | 63 | 7 | 0 | 64 | 72 | 30,18 | 88 | | NE | 1 | Cloudy. |
| | 67 | 2 | 0 | 66 | 70 | 30,18 | 83 | | NE | 2 | Cloudy. |
| 18 | 59 | 7 | 0 | 60 | 70 | 30,18 | 81 | | NE | 2 | Cloudy. |
| | 65 | 2 | 0 | 62 | 69 | 30,20 | 78 | | NE | 1 | Cloudy. |
| 19 | 59 | 7 | 0 | 59 | 69 | 30,11 | 80 | | ENE | 1 | Cloudy. |
| | 68 | 2 | 0 | 68 | 68 | 30,07 | 74 | | S | 1 | Fair. |
| 20 | 61 | 7 | 0 | 62 | 68 | 30,10 | 84 | | WSW | 1 | Cloudy. |
| | 71 | 2 | 0 | 70 | 70 | 30,08 | 66 | | NW | 1 | Fine. |
| 21 | 52 | 7 | 0 | 55 | 68 | 30,10 | 79 | | WNW | 1 | Cloudy. |
| | 66 | 2 | 0 | 65 | 68 | 30,05 | 66 | | W | 1 | Fair. |
| 22 | 50 | 7 | 0 | 52 | 66 | 30,14 | 76 | | WSW | 1 | Cloudy. |
| | 63 | 2 | 0 | 63 | 65 | 29,98 | 69 | | WSW | 1 | Cloudy. |
| 23 | 49 | 7 | 0 | 50 | 65 | 29,88 | 76 | | WNW | 1 | Cloudy. |
| | 59 | 2 | 0 | 58 | 64 | 29,96 | 65 | | NNW | 2 | Fair. |
| 24 | 48 | 7 | 0 | 50 | 63 | 30,02 | 78 | | NNW | 1 | Cloudy. |
| | 59 | 2 | 0 | 57 | 63 | 30,05 | 70 | | N | 2 | Cloudy. |
| 25 | 49 | 7 | 0 | 50 | 62 | 30,18 | 84 | | NE | 2 | Cloudy. |
| | 62 | 2 | 0 | 61 | 63 | 30,29 | 70 | | NE | 2 | Fair. |
| 26 | 45 | 7 | 0 | 47 | 61 | 30,46 | 80 | | NE | 2 | Fine. |
| | 59 | 2 | 0 | 58 | 63 | 30,47 | 70 | | NE | 1 | Fine. |
| 27 | 49 | 7 | 0 | 56 | 61 | 30,37 | 84 | | NE | 1 | Cloudy. |
| | 62 | 2 | 0 | 62 | 62 | 30,34 | 77 | | NE | 1 | Cloudy. |
| 28 | 53 | 7 | 0 | 53 | 61 | 30,34 | 88 | | NE | 1 | Cloudy. |
| | 60 | 2 | 0 | 60 | 62 | 30,32 | 68 | | NE | 1 | Cloudy. |
| 29 | 52 | 7 | 0 | 52 | 61 | 30,27 | 88 | | NE | 1 | Cloudy. |
| | 59 | 2 | 0 | 59 | 61 | 30,18 | 83 | | E | 1 | Cloudy. |
| 30 | 48 | 7 | 0 | 50 | 60 | 30,00 | 87 | | E | 1 | Foggy. |
| | 61 | 2 | 0 | 60 | 61 | 29,92 | 84 | | E | 1 | Rain. |

## METEOROLOGICAL JOURNAL

### for October, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|
| | | H. M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | ° | | | | |
| Oct. 1 | 57 | 7 0 | 57 | 60 | 29.77 | 90 | 0,077 | ESE | 1 | Rain. |
| | 64 | 2 0 | 64 | 61 | 29,76 | 87. | | NE | 1 | Cloudy. |
| 2 | 53 | 7 0 | 53 | 60 | 29.89 | 92 | 0,045 | SW | 1 | Cloudy. |
| | 64 | 2 0 | 64 | 62 | 29.96 | 86 | | SW | 1 | Cloudy. |
| 3 | 57 | 7 0 | 59 | 61 | 29,96 | 94 | | S | 2 | Cloudy. |
| | 68 | 2 0 | 68 | 63 | 29.89 | 74 | | S | 2 | Fair. |
| 4 | 52 | 7 0 | 52 | 62 | 29.97 | 90 | | S | 1 | Foggy. |
| | 63 | 2 0 | 62 | 63 | 29,92 | 77 | | S | 2 | Fine. |
| 5 | 57 | 7 0 | 57 | 62 | 29,57 | 92 | 0,142 | SW | 1 | Cloudy. |
| | 63 | 2 0 | 62 | 63 | 29,72 | 69 | | W | 1 | Fair. |
| 6 | 45 | 7 0 | 45 | 61 | 30,14 | 83 | | SW | 1 | Fine. |
| | 63 | 2 0 | 63 | 63 | 30,16 | 70 | | SW | 2 | Fair. |
| 7 | 56 | 7 0 | 57 | 62 | 30,03 | 90 | | SW | 1 | Cloudy. |
| | 61 | 2 0 | 61 | 62 | 29,90 | 80 | | S | 2 | Cloudy. |
| 8 | 46 | 7 0 | 47 | 60 | 29,77 | 85 | 0,045 | SW | 1 | Fine. |
| | 59 | 2 0 | 58 | 61 | 29,76 | 70 | | W | 1 | Fair. |
| 9 | 44 | 7 0 | 44 | 59 | 29,84 | 83 | | WNW | 1 | Fine. |
| | 56 | 2 0 | 56 | 61 | 29,95 | 68 | | NW | 2 | Fair. |
| 10 | 38 | 7 0 | 40 | 58 | 30,13 | 82 | | W | 1 | Fair. |
| | 58 | 2 0 | 56 | 60 | 30,02 | 77 | | S | 1 | Rain. |
| 11 | 53 | 7 0 | 58 | 59 | 29,52 | 95 | 0,345 | SSW | 1 | Rain. |
| | 61 | 2 0 | 61 | 61 | 29,38 | 95 | | S | 1 | Cloudy. |
| 12 | 44 | 7 0 | 46 | 59 | 29,37 | 86 | 0,350 | SW | 1 | Fair. |
| | 55 | 2 0 | 54 | 60 | 29,29 | 77 | | SW | 1 | Rain. |
| 13 | 38 | 7 0 | 38 | 58 | 29,46 | 85 | 0,037 | W | 1 | Cloudy. |
| | 52 | 2 0 | 52 | 60 | 29,52 | 78 | | W | 1 | Fair. |
| 14 | 44 | 7 0 | 52 | 58 | 29,27 | 93 | 0,023 | S | 2 | Rain. |
| | 58 | 2 0 | 58 | 60 | 29,27 | 73 | | SW | 1 | Cloudy. |
| 15 | 48 | 7 0 | 48 | 58 | 29,25 | 86 | 0,048 | SW | 1 | Fair. |
| | 55 | 2 0 | 55 | 61 | 29,33 | 68 | | WSW | 1 | Fine. |
| 16 | 39 | 7 0 | 39 | 57 | 29,61 | 84 | | WSW | 1 | Fine. |
| | 54 | 2 0 | 54 | 61 | 29,73 | 70 | | WSW | 1 | Fine. |

## METEOROLOGICAL JOURNAL

### for October, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | Time. M. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. Points. | Winds. Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | ° | ° | Inches. | ° | Inches. | | | |
| Oct. 17 | 46 | 7 | 0 | 48 | 58 | 29.67 | 88 | 0,030 | SSW | 2 | Fair. [Lightning with thunder. |
| | 60 | 2 | 0 | 59 | 60 | 29.66 | 72 | | SSW | 2 | Cloudy. |
| 18 | 46 | 7 | 0 | 48 | 58 | 29.97 | 84 | 0,063 | SW | 2 | Cloudy. |
| | 60 | 2 | 0 | 60 | 59 | 29.96 | 78 | | WNW | 2 | Cloudy. |
| 19 | 53 | 7 | 0 | 53 | 59 | 30.01 | 90 | | SSW | 2 | Cloudy. |
| | 61 | 2 | 0 | 60 | 62 | 29.96 | 76 | | S | 2 | Fair. |
| 20 | 53 | 7 | 0 | 56 | 59 | 29.85 | 86 | | S | 2 | Fair. |
| | 60 | 2 | 0 | 64 | 62 | 29.88 | 82 | | S | 2 | Fair. |
| 21 | 54 | 7 | 0 | 54 | 60 | 29.77 | 91 | 0,072 | E | 1 | Cloudy. |
| | 63 | 2 | 0 | 62 | 62 | 29.66 | 81 | | E | 1 | Cloudy. |
| 22 | 56 | 7 | 0 | 56 | 61 | 29.43 | 90 | | E | 1 | Cloudy. |
| | 59 | 2 | 0 | 57 | 62 | 29.41 | 87 | | S | 1 | Rain. The Aurora Borealis was visible from 6 to 9 P. M.; as usual, it was very brilliant, but had not much motion. |
| 23 | 41 | 7 | 0 | 41 | 60 | 29.57 | 84 | 0,290 | SW | 1 | Fair. |
| | 56 | 2 | 0 | 56 | 62 | 29.61 | 77 | | NE | 1 | Fair. |
| 24 | 44 | 7 | 0 | 44 | 60 | 29.77 | 83 | | NNE | 1 | Fine. |
| | 53 | 2 | 0 | 52 | 62 | 29.78 | 83 | | NE | 1 | Fine. |
| 25 | 45 | 7 | 0 | 46 | 60 | 29.80 | 83 | | NE | 1 | Cloudy. |
| | 53 | 2 | 0 | 53 | 60 | 29.78 | 82 | | NE | 1 | Cloudy. |
| 26 | 49 | 7 | 0 | 50 | 59 | 29.76 | 89 | | E | 1 | Fine. |
| | 60 | 2 | 0 | 58 | 61 | 29.78 | 81 | | SE | 1 | Cloudy. |
| 27 | 50 | 7 | 0 | 50 | 59 | 29.76 | 93 | 0,330 | E | 1 | Rain. |
| | 54 | 2 | 0 | 53 | 60 | 29.74 | 87 | | SE | 1 | Rain. |
| 28 | 43 | 7 | 0 | 44 | 58 | 29.68 | 90 | 0,022 | E | 1 | Foggy. |
| | 55 | 2 | 0 | 55 | 61 | 29.62 | 80 | | ESE | 1 | Fair. |
| 29 | 49 | 7 | 0 | 49 | 59 | 29.55 | 90 | | E | 1 | Cloudy. |
| | 52 | 2 | 0 | 52 | 60 | 29.52 | 90 | | E | 1 | Rain. |
| 30 | 50 | 7 | 0 | 50 | 59 | 29.30 | 90 | 0,042 | ENE | 1 | Fair. |
| | 55 | 2 | 0 | 55 | 60 | 29.18 | 80 | | E | 2 | Cloudy. |
| 31 | 50 | 7 | 0 | 50 | 59 | 29.38 | 93 | 0,085 | S | 1 | Fair. |
| | 60 | 2 | 0 | 60 | 61 | 29.55 | 81 | | S | 1 | Fair. |

## METEOROLOGICAL JOURNAL

### for November, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|
| | | H. M. | | | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | ° | | | | |
| Nov. 1 | 48 | 7 0 | 49 | 59 | 29.55 | 90 | 0,515 | S | 1 | Fine. |
| | 59 | 2 0 | 58 | 61 | 29,60 | 82 | | SW | 1 | Cloudy. |
| 2 | 51 | 7 0 | 51 | 60 | 29,73 | 90 | | S | 1 | Rain. |
| | 56 | 2 0 | 56 | 62 | 29,83 | 88 | | NE | 1 | Cloudy. |
| 3 | 47 | 7 0 | 47 | 59 | 30,23 | 85 | 0,133 | NE | 2 | Cloudy. |
| | 49 | 2 0 | 48 | 61 | 30,27 | 73 | | NE | 2 | Fine. |
| 4 | 42 | 7 0 | 42 | 58 | 30,15 | 77 | | NE | 2 | Cloudy. |
| | 45 | 2 0 | 44 | 59 | 30,03 | 77 | | NE | 2 | Fair. |
| 5 | 40 | 7 0 | 40 | 57 | 29,90 | 80 | | NE | 2 | Cloudy. [Much wind last night. |
| | 45 | 2 0 | 45 | 57 | 29,90 | 77 | | NE | 2 | Cloudy. |
| 6 | 38 | 7 0 | 38 | 55 | 29,98 | 77 | | ENE | 1 | Cloudy. |
| | 42 | 2 0 | 42 | 56 | 29,98 | 74 | | ENE | 1 | Cloudy. |
| 7 | 38 | 7 0 | 39 | 54 | 29,90 | 77 | | ESE | 1 | Cloudy. |
| | 46 | 2 0 | 46 | 55 | 29,81 | 78 | | SE | 1 | Cloudy. |
| 8 | 45 | 7 0 | 49 | 55 | 29,53 | 92 | 0,063 | SE | 1 | Rain. |
| | 54 | 2 0 | 52 | 57 | 29,41 | 88 | | ESE | 1 | Cloudy. |
| 9 | 42 | 7 0 | 42 | 56 | 29,38 | 92 | | ESE | 1 | Foggy. |
| | 52 | 2 0 | 52 | 57 | 29,43 | 91 | | ESE | 1 | Foggy. |
| 10 | 49 | 7 0 | 50 | 57 | 29,24 | 93 | 0,160 | ESE | 1 | Rain. |
| | 58 | 2 0 | 57 | 59 | 29,22 | 91 | | SW | 1 | Rain. |
| 11 | 52 | 7 0 | 52 | 58 | 29,25 | 94 | 0,563 | NE | 1 | Rain. |
| | 52 | 2 0 | 52 | 59 | 29,37 | 94 | | NE | 1 | Rain. |
| 12 | 47 | 7 0 | 47 | 57 | 29,94 | 92 | 0,310 | ENE | 1 | Rain. |
| | 56 | 2 0 | 56 | 59 | 29,96 | 89 | | S | 1 | Cloudy. |
| 13 | 51 | 7 0 | 52 | 58 | 29,70 | 92 | 0,200 | SSE | 2 | Rain. |
| | 59 | 2 0 | 59 | 60 | 29,66 | 93 | | SSW | 2 | Cloudy. |
| 14 | 51 | 7 0 | 53 | 58 | 29,57 | 94 | 0,093 | S | 2 | Rain. |
| | 54 | 2 0 | 54 | 60 | 29,55 | 94 | | S | 1 | Rain. |
| 15 | 45 | 7 0 | 45 | 58 | 29,78 | 91 | 0,375 | NE | 1 | Cloudy. |
| | 40 | 2 0 | 40 | 59 | 29,84 | 89 | | NE | 1 | Cloudy. |
| 16 | 44 | 7 0 | 44 | 58 | 29,98 | 92 | | NE | 1 | Cloudy. |
| | 48 | 2 0 | 48 | 58 | 30,00 | 92 | | NE | 1 | Rain. |

## METEOROLOGICAL JOURNAL
### for November, 1804.

| 1804 | Six's Therm least and greatest Heat. | Time. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|
| | | H. M. | ° | ° | Inches. | ° | Inches. | Points. | Str. | |
| Nov. 17 | 45 | 7 0 | 46 | 57 | 30,09 | 92 | | E | 1 | Cloudy. |
| | 50 | 2 0 | 49 | 59 | 30,12 | 86 | | E | 1 | Fair. |
| 18 | 45 | 7 0 | 46 | 57 | 30,18 | 90 | | E | 1 | Cloudy. |
| | 52 | 2 0 | 52 | 59 | 30,16 | 88 | | SE | 1 | Cloudy. |
| 19 | 48 | 7 0 | 51 | 57 | 30,10 | 93 | | S | 2 | Cloudy. |
| | 53 | 2 0 | 53 | 60 | 29,94 | 93 | | S | 1 | Rain. |
| 20 | 44 | 7 0 | 44 | 57 | 30,13 | 92 | 0,564 | SW | 1 | Cloudy. |
| | 55 | 2 0 | 50 | 60 | 30,05 | 86 | | S | 1 | Fair. |
| 21 | 48 | 7 0 | 53 | 58 | 29,50 | 88 | 0,275 | WSW | 2 | Cloudy. |
| | 53 | 2 0 | 51 | 60 | 29,76 | 73 | | W | 2 | Fair. |
| 22 | 44 | 7 0 | 45 | 57 | 29,76 | 80 | | W | 1 | Fine. |
| | 48 | 2 0 | 48 | 58 | 29,89 | 73 | | NW | 1 | Fair. |
| 23 | 38 | 7 0 | 42 | 57 | 29,97 | 85 | | WSW | 1 | Cloudy. |
| | 50 | 2 0 | 50 | 58 | 29,84 | 85 | | SW | 1 | Rain. |
| 24 | 39 | 7 0 | 39 | 56 | 29,85 | 87 | 0,235 | ENE | 1 | Rain. |
| | 39 | 7 0 | 37 | 57 | 29,80 | 93 | | NE | 1 | Rain. |
| 25 | 36 | 7 0 | 43 | 55 | 29,84 | 81 | 0,510 | E | 2 | Rain. |
| | 44 | 2 0 | 44 | 56 | 29,89 | 76 | | E | 2 | Cloudy. |
| 26 | 35 | 7 0 | 37 | 53 | 30,04 | 80 | | NE | 2 | Cloudy. |
| | 44 | 2 0 | 44 | 56 | 30,08 | 80 | | NE | 1 | Fair. |
| 27 | 35 | 7 0 | 36 | 52 | 30,00 | 91 | | NE | 1 | Cloudy. |
| | 41 | 2 0 | 41 | 55 | 30,05 | 77 | | NE | 1 | Fair. |
| 28 | 37 | 7 0 | 37 | 52 | 30,02 | 77 | | ENE | 2 | Cloudy. |
| | 36 | 2 0 | 36 | 53 | 29,98 | 79 | | ENE | 2 | Cloudy. |
| 29 | 34 | 7 0 | 35 | 51 | 29,98 | 88 | | NE | 1 | Cloudy. |
| | 38 | 2 0 | 38 | 53 | 29,97 | 83 | | NE | 1 | Cloudy. |
| 30 | 34 | 7 0 | 34 | 50 | 29,95 | 82 | | NE | 1 | Fair. |
| | 40 | 2 0 | 40 | 51 | 29,90 | 81 | | NE | 1 | Cloudy. |

## METEOROLOGICAL JOURNAL
### for December, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | M. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. Points. | Str. | Weather. | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | ° | ° | Inches. | | Inches. | | | | |
| Dec. 1 | 35 | 8 | 0 | 35 | 50 | 29,96 | 90 | | NE | 1 | Cloudy. | |
| | 42 | 2 | 0 | 42 | 53 | 30,05 | 83 | | NE | 1 | Cloudy. | |
| 2 | 35 | 8 | 0 | 35 | 50 | 30,28 | 90 | | NE | 1 | Cloudy. | |
| | 39 | 2 | 0 | 38 | 53 | 30,30 | 88 | | NE | 1 | Cloudy. | |
| 3 | 36 | 8 | 0 | 37 | 51 | 30,33 | 86 | | E | 1 | Cloudy. | |
| | 38 | 2 | 0 | 37 | 53 | 30,31 | 80 | | E | 1 | Fine. | |
| 4 | 28 | 8 | 0 | 29 | 49 | 29,98 | 84 | | E | 1 | Fine. | |
| | 39 | 2 | 0 | 39 | 51 | 29,78 | 83 | | E | 1 | Cloudy. | |
| 5 | 39 | 8 | 0 | 45 | 51 | 29,33 | 93 | 0,035 | E | 1 | Foggy. | |
| | 48 | 2 | 0 | 48 | 53 | 29,08 | 94 | | E | 1 | Cloudy. | |
| 6 | 37 | 8 | 0 | 37 | 51 | 29,05 | 95 | | S | 1 | Foggy. | |
| | 41 | 2 | 0 | 41 | 53 | 29,11 | 94 | | S | 1 | Foggy. | |
| 7 | 38 | 8 | 0 | 43 | 52 | 29,43 | 94 | | NNE | 1 | Cloudy. | |
| | 45 | 2 | 0 | 45 | 54 | 29,54 | 91 | | NNE | 1 | Cloudy. | |
| 8 | 43 | 8 | 0 | 43 | 52 | 29,83 | 93 | | NE | 1 | Cloudy. | |
| | 45 | 2 | 0 | 45 | 54 | 29,90 | 90 | | ENE | 1 | Cloudy. | |
| 9 | 40 | 8 | 0 | 40 | 52 | 29,88 | 90 | | E | 1 | Fair. | |
| | 45 | 2 | 0 | 45 | 53 | 29,86 | 90 | | SE | 1 | Rain. | |
| 10 | 40 | 8 | 0 | 40 | 53 | 29,94 | 94 | 0,093 | | | Foggy. | |
| | 47 | 2 | 0 | 47 | 54 | 29,92 | 94 | | S | 1 | Cloudy. | |
| 11 | 44 | 8 | 0 | 44 | 53 | 29,78 | 94 | | S | 1 | Fair. | |
| | 50 | 2 | 0 | 50 | 55 | 29,71 | 93 | | S | 1 | Cloudy. | |
| 12 | 41 | 8 | 0 | 41 | 53 | 29,52 | 92 | 0,112 | SSW | 2 | Fine. | |
| | 51 | 2 | 0 | 47 | 55 | 29,50 | 88 | | S | 1 | Cloudy. | |
| 13 | 46 | 8 | 0 | 46 | 53 | 29,06 | 90 | 0,030 | S | 2 | Fair. | [ Much wind this night. |
| | 48 | 2 | 0 | 48 | 56 | 29,07 | 83 | | S | 2 | Fine. | |
| 14 | 44 | 8 | 0 | 44 | 54 | 29,31 | 83 | 0,030 | WSW | 1 | Fair. | |
| | 48 | 2 | 0 | 48 | 50 | 29,49 | 80 | | WNW | 1 | Fair. | |
| 15 | 42 | 8 | 0 | 43 | 54 | 29,78 | 83 | | WNW | 2 | Cloudy. | |
| | 47 | 2 | 0 | 47 | 58 | 29,92 | 81 | | NW | 2 | Fine. | |
| 16 | 35 | 8 | 0 | 35 | 53 | 30,10 | 87 | | N | 1 | Fine. | |
| | 41 | 2 | 0 | 41 | 56 | 30,10 | 83 | | NNW | 1 | Cloudy. | |

## METEOROLOGICAL JOURNAL

### for December, 1804.

| 1804 | Six's Therm. least and greatest Heat. | Time. H. | Time. M. | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. Points. | Winds. Str. | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | | | ° | ° | Inches. | | Inches. | | | |
| Dec. 17 | 32 | 8 | 0 | 32 | 52 | 29.95 | 83 | | NE | 1 | Fair. |
| | 35 | 2 | 0 | 35 | 55 | 29.96 | 83 | | NE | 2 | Fair. |
| 18 | 31 | 8 | 0 | 32 | 51 | 30.15 | 87 | | NE | 2 | Cloudy. |
| | 37 | 2 | 0 | 37 | 54 | 30.21 | 84 | | NE | 2 | Fair. |
| 19 | 29 | 8 | 0 | 29 | 49 | 30.39 | 87 | | ENE | 2 | Snow. |
| | 31 | 2 | 0 | 30 | 51 | 30.40 | 84 | | NE | 2 | Snow. |
| 20 | 28 | 8 | 0 | 28 | 48 | 30.25 | 82 | | ENE | 2 | Cloudy. [Much wind last night. |
| | 32 | 2 | 0 | 30 | 49 | 30.18 | 79 | | ENE | 2 | Fair. |
| 21 | 26 | 8 | 0 | 38 | 46 | 30.04 | 85 | | NE | 1 | Fine. |
| | 35 | 2 | 0 | 35 | 50 | 30.02 | 81 | | NE | 1 | Fine. |
| 22 | 33 | 8 | 0 | 34 | 46 | 29.84 | 83 | | NNE | 2 | Cloudy. |
| | 35 | 2 | 0 | 35 | 49 | 29.60 | 82 | | NNE | 2 | Cloudy. |
| 23 | 29 | 8 | 0 | 32 | 46 | 29.57 | 88 | | ENE | 1 | Snow. |
| | 35 | 2 | 0 | 35 | 48 | 29.64 | 90 | | S | 1 | Cloudy. |
| 24 | 19 | 8 | 0 | 20 | 44 | 29.76 | 88 | | E | 1 | Fair. |
| | 32 | 2 | 0 | 30 | 47 | 29.72 | 88 | | NE | 1 | Fair. |
| 25 | 30 | 8 | 0 | 35 | 45 | 29.63 | 90 | | NE | 1 | Cloudy. |
| | 38 | 2 | 0 | 38 | 47 | 29.55 | 91 | | E | 1 | Cloudy. |
| 26 | 35 | 8 | 0 | 35 | 45 | 29.47 | 93 | 0,130 | E | 1 | Cloudy. |
| | 37 | 2 | 0 | 37 | 48 | 29.47 | 93 | | NE | 1 | Cloudy. |
| 27 | 33 | 8 | 0 | 34 | 45 | 29.55 | 91 | 0,075 | NE | 1 | Cloudy. |
| | 37 | 2 | 0 | 37 | 48 | 29.60 | 90 | | NE | 1 | Cloudy. |
| 28 | 34 | 8 | 0 | 34 | 45 | 29.69 | 84 | | NE | 2 | Cloudy. |
| | 35 | 2 | 0 | 34 | 48 | 29.68 | 82 | | NE | 2 | Cloudy. |
| 29 | 31 | 8 | 0 | 32 | 45 | 29.80 | 80 | | NE | 2 | Cloudy. |
| | 34 | 2 | 0 | 33 | 47 | 29.96 | 77 | | NE | 2 | Cloudy. |
| 30 | 27 | 8 | 0 | 27 | 44 | 30.18 | 81 | | NE | 1 | Fine. |
| | 32 | 2 | 0 | 32 | 47 | 30.16 | 82 | | ENE | 1 | Fine. |
| 31 | 28 | 8 | 0 | 32 | 44 | 30.06 | 92 | | ENE | 1 | Fine. |
| | 36 | 2 | 0 | 36 | 48 | 30.01 | 74 | | ENE | 2 | Fine. |

| 1804. | Six's Therm. without. | | | Thermometer without. | | | Thermometer within. | | | Barometer.* | | | Hygrometer. | | | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Greatest height. | Least height. | Mean height. | Greatest height. | Least height. | Mean height. | Greatest height. | Least height. | Mean height. | Greatest height. | Least height. | Mean height. | Greatest height. | Least height. | Mean height. | |
| | Deg. | Deg. | Deg. | Deg. | Deg. | Deg. | Deg. | Deg. | Deg. | Inches. | Inches. | Inches. | Deg. | Deg. | Deg. | Inches. |
| January | 55 | 27 | 45,0 | 55 | 28 | 45,5 | 59 | 47 | 54,4 | 30,18 | 28,68 | 29,60 | 97 | 77 | 88,8 | 1,673 |
| February | 51 | 25 | 38,9 | 51 | 26 | 38,9 | 59 | 48 | 51,7 | 30,51 | 29,05 | 30,03 | 95 | 69 | 83,0 | 1,337 |
| March | 62 | 28 | 43,2 | 62 | 28 | 43,4 | 60 | 48 | 53,7 | 29,92 | 29,15 | 29,63 | 96 | 68 | 83,9 | 1,539 |
| April | 71 | 34 | 46,3 | 70 | 36 | 46,8 | 61 | 52 | 55,5 | 30,28 | 29,22 | 29,71 | 94 | 65 | 81,0 | 1,604 |
| May | 73 | 44 | 59,6 | 73 | 42 | 60,2 | 67 | 60 | 62,7 | 30,29 | 29,64 | 29,92 | 92 | 64 | 76,7 | 1,248 |
| June | 87 | 47 | 64,7 | 85 | 50 | 64,3 | 71 | 62 | 65,9 | 30,40 | 29,68 | 30,11 | 88 | 62 | 75,6 | 0,522 |
| July | 80 | 49 | 62,8 | 79 | 53 | 63,6 | 69 | 62 | 65,8 | 30,23 | 29,44 | 29,84 | 94 | 60 | 75,6 | 3,705 |
| August | 81 | 49 | 63,2 | 81 | 50 | 63,2 | 70 | 62 | 65,4 | 30,30 | 29,18 | 29,88 | 93 | 62 | 76,2 | 2,801 |
| September | 81½ | 45 | 61,7 | 81 | 47 | 62,2 | 74 | 60 | 66,8 | 30,47 | 29,88 | 30,13 | 91 | 63 | 77,5 | |
| October | 68 | 38 | 53,6 | 68 | 38 | 53,8 | 63 | 57 | 60,3 | 30,16 | 29,18 | 29,70 | 95 | 68 | 83,4 | 2,046 |
| November | 59 | 34 | 45,9 | 58 | 34 | 46,3 | 62 | 50 | 57,0 | 30,27 | 29,22 | 29,84 | 94 | 73 | 85,7 | 3,993 |
| December | 51 | 19 | 37,1 | 50 | 20 | 37,5 | 58 | 44 | 51,4 | 30,40 | 29,86 | 29,80 | 95 | 74 | 86,9 | 0,505 |
| Whole year | | | 51,8 | | | 52,1 | | | 59,2 | | | 29,85 | | | 81,2 | 20,973 |

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

*Variation of the Magnetic Needle,*
1804.

| | | |
|---|---|---|
| March | - - - | 24°. 11′.8 West. |
| June | - - - | - 24°. 8′.4 |
| July | - - - | 24°. 10′.8 |
| September | - - | - 24°. 11′.3 |
| December | - - | 24°. 11′.4 |